HUMAN BIOLOGY
a record of research
DECEMBER, 1948
VOL. 20 No. 4

THE RELATION BETWEEN INBREEDING AND INTELLIGENCE

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THE effect of inbreeding upon man and animals is still one of the question marks of biology. In general, the opinion of geneticists seems to be that expressed by Lush (1) who considers that the result depends upon the soundness of the stock upon which inbreeding is practised. It concentrates genes more rapidly than is the case with other types of matings, thus increasing the degree of homozygosis. It does this with genes for desirable characters and with those for undesirable ones. As the latter may undo the good work of the former it is not surprising that inbreeding so often results in disaster. All writers on the subject emphasize that sound stock to begin with, together with rigid culling of animals with undesirable genes as they come to light, are essentials for successful inbreeding. On the other hand, many practical breeders believe that inbreeding has a weakening effect upon the line independent of the gene concentration, a difficult belief to prove when so few genes are actually known.

There is evidence that the animals which have proved outstanding for production in several breeds of cattle have not been produced by the concentration of genes by inbreeding. Thus, McPhee and Wright (2, 3) found that the 20 prize-winning beef Shorthorn sires and the 100 highest milk yielding Shorthorn cows were no more inbred than

the average of the breed, and Asdell (4) has found the same for the 100 highest milk yielding Holsteins and the 100 highest butter-fat yielding Guernseys. In the latter work it was concluded that the best advice still is to breed the best to the best and to rely upon this to produce the most desirable combinations. No definite system of breeding was outstanding in its contribution to the select class of animals examined. In the case of the Holsteins many of them showed a reduction of inbreeding when highly inbred parents were crossed, a result due to the fact that part of the inbreeding in the sire and dam was not to the same ancestor. This is the only exception to the statement made above regarding the lack of breeding systems which produce the outstanding individuals.

Evidence that characters which need for their highest expression the interaction of many factors, health, development of specific parts or organs, ability to eat much, and good digestive ability, are best expressed with a condition of heterosis, or outbreeding, is not lacking, especially in pigs. Winters *et al.* (5) have found better production when inbred lines of pigs have been crossed, so that the degree of inbreeding has been markedly reduced or has disappeared altogether. But the inbred lines used for the cross must be good for the cross to be good. This may mean that homozygosis is not desirable for the best results to be achieved; it does not necessarily mean that inbreeding is harmful apart from its genetic effects. The cattle work quoted above seems to point in the same direction, and in an extensive cattle cross-breeding experiment by Fohrman (6) marked improvement in milk yield has resulted, apparently as a result of the crossing. Much more work could be cited, some pointing in one direction and some in the other. There are too many unknowns in all the data to draw a general conclusion; so much depends upon unknown factors in the genetic make-up of the animals used.

In man there has been a general prejudice against inbreeding of any kind, a prejudice which may have been founded upon observations on the ill effects of inbreeding as practised without culling or selection, and which has found expression in the many ecclesiastical and civil laws restricting consanguineous marriages. Huth (7) has reviewed much of the literature, legal and otherwise, but he points to a general trend of opinion in the opposite direction, a trend toward *laissez faire*. This is aptly summed up in his words:

> ". . . . there is a widely spread uncomfortable feeling that consanguineous marriages are to be avoided if possible; but if Angelina insists on having

> Edwin, and Edwin is fortunate enough to make Angelina persist in her intention, why, let them marry and may they be happy!"

The modern tendency has been to reduce the number of legal restrictions, making more inbreeding possible. Probably the gradual lifting of these restrictions has had little effect upon the quality of humanity in general; the first cousin marriages allowed represent only a mild degree of inbreeding and they are not common in any case, less than 1 per cent, according to the data given by Bell (8), while, according to Haldane (9), they have decreased with increased transportation facilities.

Mental ability is a form of production which is inherited. Numerous papers have appeared to show that this is so and it is unnecessary to review them. The purpose of this inquiry is to obtain data on the mental ability of inbred persons and to determine, if possible, the cause of any deviation from the average in such groups. It requires a variety of other conditions, such as health, and so on, to show its highest expression in activity and so it falls into the class of multiple interacting factors under discussion.

In 1926 Woods (10) wrote a book entitled *Mental and Moral Heredity in Royalty, A Statistical Study in History and Psychology.* In this work he attempted to show that genius and feeble-mindedness appeared in groups. A royal house would have several closely related worthy members at one period of its history. Then the genius would be lost due to unsuitable matings. The same was shown to be true for feeble-minded groupings with recovery if the matings were satisfactory. In making this analysis he classified about 700 persons of royal birth together with a few of the higher aristocracy closely related to the wives of royalty. The ratings were made for intellectual worth or fitness for their duty on the basis of the articles about them in standard biographies and other historical sources. As Woods says, "The adjectives that are used by biographers and historians are the basis of the estimate." He arranged his people, both men and women, in ten classes, the lowest, I 1, containing the imbeciles and feeble-minded, and the highest, I 10, the most intelligent or successful as rulers or leaders. The group, I 1, is small and it contains nine names, among them, Ivan and Feodor of Russia, sons of Alexis, and Charles II of Spain. Group I 10 is also small, containing fourteen names, among them, Frederick the Great of Prussia and his sister, Louise Ulrica, Queen of Sweden, Gustavus Vasa of Sweden, Louis II the

Great Condé, William the Silent of Orange, and Catherine II, the Great, of Anhalt-Russia.

While one can differ in the placing of individuals in these ten classes the general classification seems to have been fairly done and differences of opinion on individual placings would probably cancel one another. For the purpose of the present writer the classification had the great advantage that from his point of view the material used was entirely objective. He had no hand in making the classification.

There are a number of small errors and duplications in Woods' material. When these were eliminated there remained 690 individuals who could be used in this study. Their numerical distribution into classes is given in Table 1.

TABLE 1

Distribution of persons into grades of intellect and their average coefficients of inbreeding

GRADE	WOODS' DISTRIBUTION	BINOMIAL DISTRIBUTION	NUMBER STUDIED	AVERAGE INBREEDING COEFFICIENT
Low 1	9	1.4	9	4.9
2	26	12.1	26	5.4
3	50	48.5	47	4.3
4	96	113.2	87	2.9
5	162	169.8	150	3.1
6	125	169.8	118	2.8
7	111	113.2	99	2.4
8	67	48.5	54	2.1
9	30	12.1	28	1.1
High 10	14	1.4	14	2.7
Total	690		632	

Examination of this data for goodness of fit to a normal distribution curve revealed a fairly good fit except at the ends of the curve and this is understandable since very poor and very successful rulers are more likely to receive notice than those who do an average job. They are therefore present in greater numbers than would be expected in a random distribution. This is especially apparent in the extreme grades, I 1, and I 10. Theoretically, grades I 5 and I 6 should be equal in numbers but I 5 is increased at the expense of I 6, showing that Woods

was unduly severe in his judgment of average specimens of royalty. These differences from random distribution do not impair the value of the data for the use to which it is now put.

Six-generation pedigrees were constructed for all these people. In some cases it was not possible to construct them as genealogical data could not be obtained with the resources at the writer's disposal. These cases have been omitted from further consideration. A criticism which may be made is that the official pedigree may not be accurate in fact; that the morals were such in some courts that parentage is problematical. Most of the cases are drawn from comparatively modern times, in an era of unusually frank memoirs, when the doings of the people considered were subject to close scrutiny. When contemporary gossip has cast doubt upon the parentage of an individual, that case has also been omitted from further consideration. The remaining pedigrees can probably be regarded as reliable. Altogether, 632 persons of known pedigree remain for further treatment. The coefficient of inbreeding of each was calculated, using S. Wright's (11) short formula,

$$F_x = \Sigma\,(\tfrac{1}{2})^{n+n^1+1}$$

where F_x is the coefficient of inbreeding; n is the number of steps from the father to the common ancestor, and n^1 the number of steps from the mother. The correction for the coefficient of inbreeding of a common ancestor was not used since it was found not to affect the result. All coefficients were multiplied by 100 and calculated to one decimal place. This method is sufficient to reveal any inbreeding in a 6-generation pedigree except that a single common ancestor in the sixth generation fails to give a significant figure and is thus counted as zero inbreeding. The results were averaged for each intelligence grade and are shown in Fig. 1.

This graph reveals, in general, a progressive decrease in average inbreeding as the intelligence increases. The coefficient for I 1, which is less than that for I 2, may be due to the small sample, or, more likely, to the fact that imbecility may be due to a variety of causes, such as a cretinoid condition, not necessarily due to hereditary mechanisms. Likewise, the increased degree of inbreeding in I 10 as compared with that for I 9 may be due to the smallness of the sample or it may indicate a concentration of genetic factors by inbreeding which causes genius. The writer does not believe that the latter is the cause

in this material since, if it were, one would expect the upper grades, I 8 and I 9 to share in the increase. The concentration of some of the factors should make their influence felt in these grades. Another factor which enters here is the one pointed out by Woods, the concentration of famous names in a family tree. Thus, the grade I 10, contains Frederick the Great of Prussia and his sister, each with 8.3 per cent of inbreeding and Louis II, the Great Condé, with his sister, Anne, Duchess of Longueville, each with 6.6 per cent. These four

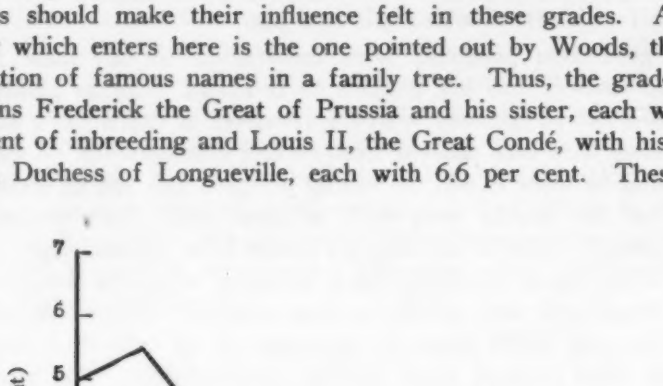

FIG. 1. RELATION OF PERCENTAGE OF INBREEDING TO INTELLIGENCE

account for most of the upswing at I 10. The part of the curve between I 2 and I 9, inclusive, shows a steady decline in the degree of inbreeding as intelligence increases. Even if the curve is corrected by calculating the persons of unknown pedigree as zero inbred (and difficulty in constructing the pedigree nearly always indicates this) its trend is not altered; it is merely dropped two decimal points throughout.

This curve can be, and is, in fact, misleading. It can be interpreted as indicating that inbreeding is the cause of the lessened intelligence. But there is the possibility that inbreeding was practised only by those of lower intelligence in the first place. This possibility has been

tested. Woods showed that the coefficient of correlation (r) between the intelligence ratings of husband and wife was $0.08 \pm .076$, indicating that as far as intelligence of the husband and wife are concerned mating was random and not assortive. Since the main reasons for choice of mate in royal houses are to provide heirs and to satisfy political reasons, the marriage arrangements rest with the house of which the male is a part, so they, alone, therefore, need be considered. It may be argued that the young man has little say in the matter, that his parents, near relatives, and their advisers make the selection, and they should be the subjects of the test. But they do not provide the offspring of the mating, so, biologically, the husband must be the subject. The wife may be neglected as, statistically speaking, she represents the average intelligence of the group under study. So, therefore, a comparison was made of the intelligence of the husbands whose marriages produced no inbreeding with that of an equal number of husbands whose matings produced a maximum of inbreeding. In other words, the children were not considered, but only their fathers. Only legitimate matings were considered since illegitimate children are rarely considered in Woods' data unless they were outstanding, so that matings which produce them, usually with zero inbreeding, cannot be considered as biologically randomized for our purpose. There remained 50 matings with zero inbreeding to be considered. The husbands had an average rating of I 6.2. For the highly inbred matings all of 6.0 per cent inbreeding and above were taken. This lower limit was chosen since it gave a sample of approximately equal size. There were 52 such matings and the average intelligence of the fathers was I 4.9. The fathers of the more highly inbred children averaged, therefore, more than a whole intelligence grade below the fathers of the children that were not inbred. The decline in intelligence rating of the children with increasing inbreeding may therefore be due to the poorer quality of the fathers who practised the inbreeding.

Is this the whole explanation for the decline? May the inbreeding in itself produce some of the effect? A method of testing this possibility is to work with fathers within one intelligence grade and to decide whether the more inbred children are less intelligent than the less inbred ones. In order to test this possibility the material was sorted according to the intelligence grade of the fathers and then divided into approximately two equal halves, the less and the more inbred. The average intelligence of the children was then compared in each group.

No consistent trend was found (Table 2), perhaps because the dividing line made the average coefficient of inbreeding in the more inbred groups very low. The amount of inbreeding might not have been sufficient to show a difference in intelligence. Accordingly, the averages of the 10 or more (the division was made to avoid separating those with the same tenth per cent of inbreeding) most inbred children in each father intelligence group were considered (Table 2). These showed a

TABLE 2

Effect of inbreeding within intelligence grades of fathers

FATHERS' GRADES	LESS INBRED HALF		MORE INBRED HALF		THE "TEN" MOST INBRED	
	N	Average grade of children	N	Average grade of children	Average grade	Average grade of mothers
1	..	..	..	..	..	..
2	14	4.3	12	5.2	..	..
3	35	5.8	35	5.1	4.6	6.0
4	37	5.1	39	5.3	4.5	4.7
5	49	5.2	48	5.6	4.5	?
6	34	5.6	30	5.5	5.0	5.7
7	46	6.0	69	5.9	4.8	5.6
8	27	6.5	25	5.7	5.6	5.3
9	9	4.9	9	5.4	..	..
10	13	6.8	4	6.8	..	..

consistent trend toward less intelligence. But, working with such small groups as 10 individuals, it is not safe to assume that the mothers were average, as had been done in the larger groupings on the basis of the correlation coefficient. The intelligence of all the mothers was not known but the average grades of the known ones did support the theory of a decline due to the inbreeding. The results, however, might equally well be explained by invoking the principle of reversion to the average which must be taken into account in considering material of this nature. Or, the classification may have merely picked out cases in which Woods' grading was awry. The evidence is far too scanty to warrant the conclusion that the inbreeding in itself, without the influence of parental genes, causes a decline in intelligence.

The sample of humanity studied shows a steady decline in intelligence as inbreeding increases but this is in part, if not wholly, due to the poorer quality of the fathers who have practised inbreeding. Evidence that inbreeding has a deleterious effect other than that of concentration of genes which produce undesirable effects is lacking. This result is entirely in line with general belief in regard to the transmission of hereditary traits. It adds strong evidence to support our existing knowledge that mental ability, or the lack of it, is inherited.

In discussions of this work with others the writer has been asked repeatedly about the Ptolemies. Are they not an example of close inbreeding without ill effects? They are popularly believed to have practised brother and sister matings for generations. The writer has used the historical data given by Mahaffy (12) to inquire into this question with the following result: Ptolemy I, Soter, the first of the line, was highly successful and might have received the rating I 9 in Woods' scheme. His heir, Ptolemy II, Philadelphus, was not inbred and he might have had the rating I 7. The next, Ptolemy III, Euergetes, was also the result of an outcross but his intelligence or success seems to have been low, meriting about I 3. He married outside his line but his son, Ptolemy IV, Philopater, was a very poor specimen, rating about I 2. He married his sister, Arsinoe, who was fairly able, rating about I 7. The son, Ptolemy V, Epiphenes, was somewhat better than his father, but not much is known of him. He might rate I 4. He married Cleopatra I of Syria, an able woman, not closely related. One son, Ptolemy VII, Philometor, was average, about I 6, and he married his sister, Cleopatra II, who was an able woman. Their son did not continue the dynasty. The younger son of Ptolemy V, Ptolemy IX, Physkon, was poor, about I 2. He married his niece, Cleopatra III, and the son who carried on the dynasty, Ptolemy XI, was poor, I 3. His son, Ptolemy XIII, Auletes, had for mother a woman whose ancestry is unknown to history, and therefore she was probably unrelated to her husband who was a weak king, I 2. Ptolemy XIII was father of Cleopatra the Great by an unknown woman. She was, therefore, the result of an outcross in two generations and she cannot be quoted in any discussion of inbreeding. The inbreeding occurred during a time when the rulers were weak men and the results of these matings were about what one would expect, considering the stock from which they came. Intellectual capacity had disappeared before the inbreeding. It cannot be used as an argument for the

inheritance of strength because the inbred Ptolemies were weak, and it cannot be quoted as an example for illustrating the evil effects of inbreeding as the stock was already poor.

An interesting sideline on this work is concerned with the question of the number of ancestors a royal person has, compared with the number he might have, for example, in the tenth generation back. For instance, George V of England possessed no more than 285, and Edward VII, 238 separate ancestors in this generation instead of the possible 1,024. This reduction in numbers does not necessarily represent much inbreeding since duplications which produce inbreeding must be common to the father's and to the mother's pedigree, while inbreeding more than 6 generations back produces little effect upon the concentration of genes. George V was 0.9 per cent inbred and Edward VII, 6.8 per cent inbred in six generations, numbers which are out of proportion to the reduction in their ancestors.

CONCLUSIONS

1. Analyses of royal pedigrees show a progressive decline in intelligence as inbreeding increases.

2. The men whose matings produced the inbreeding were of less average intelligence than were those who married into families less related to themselves.

3. The evidence available indicates that the decline of intelligence with inbreeding is probably the result of the poor stock which has practised this form of marriage.

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RECENT CHANGES IN THE SECONDARY SEX RATIO OF THE UPPER SOCIAL STRATA

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IN all Western European countries and the United States, life, during the last one hundred years, has begun with an excess of males. For every one hundred female births, one hundred and five or one hundred and six male births have been reported. In addition it has been found that during pregnancy more male than female fetuses abort, at least during those months of pregnancy when it is possible to determine the sex of the aborted fetus.

The above inequalities in favor of male offspring have lead S. Winston in his paper "The influence of social factors on the sex ratio at birth" (1) to arrive at the following important conclusion: "The sex ratio at conception is significantly higher than the sex ratio at birth . . . . the lesser percentage of males at birth than at conception is accounted for by a differential prenatal mortality bearing more heavily upon the male fetus than upon the female fetus."

Starting out from this hypothesis, Winston (p. 10) concludes that "any factors which operate to diminish prenatal mortality tend to raise the sex ratio at birth." "These factors," he continues, "do not produce a raised sex ratio by means of sex reversal, but rather their role is a sex-eliminating role." Thus Winston expects in the upper social classes, where there are fewer abortions and stillbirths, a higher percentage of male births than in the total population. Using the genealogical records of 5,466 completed families, the father being a descendant of early American settlers, Winston finds that of 15,763 children, 8,329 or 52.84 per cent were male, *i. e.* the sex ratio here is 112.0 as against a sex ratio of 105.8 for the United States as a whole. For two-child families only, Winston obtains the highest sex ratio in his data, *i. e.* 121.3 as against a sex ratio of "only" 106.2 for families of five or more children. In a recent paper F. A. Woods (2) finds a sex ratio of

137 for the royal families of Europe, the highest sex ratio on record for a special social group. We computed for Winston's data a difference of 1.43 per cent between the percentage of male births here and the United States as a whole, and the difference is 3.6 times as large as its standard error, and therefore statistically significant.

Commenting on the lower sex ratio of the large families of his data, Winston writes (p. 19): "In general, then, as the size of the family increases, the 'normal' sex ratio is approached by the socially select group under consideration." This contention must be disproved by us, lest it be argued that Winston's sample of upper class families was mainly a sample of small families, and that it was the size of these families rather than their social status which brought about the high sex ratio observed. Unfortunately Winston does not break down the data on his large families, and therefore we do not know whether the lower sex ratio in the larger families results from a much lower sex ratio in the later born children or whether the sex ratio is low also for the first and second child in a large family. Earlier investigators (3) have shown that the chances for a male birth decrease with each successive pregnancy, but they used data derived from the general population.

It might also be argued that the families of Winston's study formed too homogeneous a racial group, and that Winston had shown only that for Anglo-Saxons the sex ratio is higher than for other racial groups. For that reason the present study utilizes both American and German data, neither of which come from a racially homogeneous group.

The American families represent the families of men listed in *Who is Who in Commerce and Industry* (4), and since it has been found by Savorgnan (5), that the sex ratio of a nation may rise temporarily during wartime, the 1940 rather than the 1945 edition of this book was used. Also, since many leaders in American business and industry have started out in low paid positions, working their way to the top, the families of only those men were used who had enjoyed a four-year college education, so that a favorable social condition prevailed during the early years of marriage.

The families of foreign-born men were utilized only in case their marriage took place in the United States and all the children were born in the United States. Adopted children and twin births of like sex were omitted. Cases of multiple marriages were included only if the listing made it clear which children were born to which mother.

No marriages entered into after 1927 were included, as it was felt that these families may not have been completed by 1940. Also no marriages started before 1900 were included, as this would have introduced a bias in favor of long-lived families. Families whose marriages were entered into between 1915 and 1918 inclusive were kept separate, as it was felt that at least one child of such a marriage was born during the war period, and families whose marriages were entered into between 1910 and 1914 inclusive were omitted, since it could not be determined whether any children of such marriages were born during the first World War.

The sample of marriages entered into between 1900 and 1909 inclusive and the sample of marriages entered into between 1919 and

TABLE 1

Sex ratio at birth among four groups of upper class Americans

DATA	NUMBER OF FAMILIES	NUMBER OF CHILDREN	AVERAGE NO. OF CHILDREN PER FAMILY	MALE OFFSPRING		
				Number	Per cent	Sex ratio
Winston's	5 468	15 763	2.88	8 329	52.84	112.0
New Com. and I.	618	1 541	2.49	824	53.41	114.6
War Com. and I.	600	1 545	2.58	799	51.72	107.1
Old Com. and I.	600	1 624	2.71	839	51.67	106.9

1927 inclusive were not combined. Winston (1) in the concluding paragraph of his paper states that, if social factors influence the secondary sex ratio of man, then, with an improvement of living conditions in the United States, we may expect in the future a significantly higher sex ratio for the entire population. We have thus three sets of data, which we refer to as Old Com. and I. (marriages 1900-1909), War Com. and I. (marriages 1915-1918), and New Com. and I. (marriages 1919-1927).

In Table 1 the sex ratios for the three sets of data of the present study are listed, together with Winston's data for comparison. It may be seen that the average number of children per family varies considerably among the four sets of data listed here. Since Winston had

found, for his data, a significant effect of the family size on the secondary sex ratio, the sex ratios of the four sets of data cannot be compared without some correction.

In Table 2, small families (one to three children) and large families (four or more children) are listed separately. It may be seen that, for small families, there is little difference, if any, between size and sex ratio of Winston's data and our New Com. and I. data (families with marriages started between 1919 and 1927); both give

TABLE 2

The sex ratio of small and large families of upper class Americans

DATA	NUMBER OF FAMILIES		AVERAGE FAMILY SIZE		OFFSPRING			
					Total		Percentage male births	
	Small	Large	Small	Large	Small	Large	Small	Large
Winston's	3 852	1 614	2.01	4.96	7 761	8 002	53.8	51.9
New Com. and I.	509	110	2.07	4.44	1 055	486	53.7	52.7
War Com. and I.	487	113	2.14	4.47	1 040	505	51.5	52.1
Old Com. and I.	461	139	2.16	4.47	997	621	52.7	50.1

us about 53.5 per cent male births. We may therefore assume that this percentage of male births which corresponds to a sex ratio of about 115 male to 100 female births is about "normal" for samples of small families of the upper social strata.

In Winston's data and the Old Com. and I. data there are two to two-and-one-half per cent fewer male births in the large families than in the small families; however for the New and War Com. and I. data the difference in the percentage of male offspring between small and large families is statistically insignificant. In addition, large families constitute a greater proportion of the total data of both Winston's and the Old Com. and I. data than for the War and New Com. and I. data. Since the average size of the upper class family has been constantly decreasing during the twentieth century (Winston

(6)), we may assume that data with a higher average family size cover an earlier period than data with a lower average family size. Thus it appears that Winston's data cover a period which extends further back than the War and New Com. and I. data. The book from which Winston obtains his data was published thirteen years before *Who is Who in Commerce and Industry* was published.

Thus there appears to be a definite trend in the twentieth century towards an increased sex ratio in large families of the upper social strata. To further test this hypothesis, an additional sample of large families was drawn from the source book (*Who is Who in Commerce and Industry*) which included (a) old families with four or more children where the marriage had occurred between 1900 and 1909 inclusive, and (b) new families with four or more children where the marriage had occurred between 1919 and 1927 inclusive. Sets (a) and (b) each consist of two hundred families, and in each seventy-two fathers had not gone to college, but by the time the younger children were being born, these families also belonged financially to the upper social class. These data are tabulated in Table 3.

TABLE 3

The sex ratio in large families of upper class Americans*

DATA	NUMBER OF FAMILIES	AVER. NO. OF CHILDREN PER FAMILY	NUMBER OF MALE CHILDREN	PERCENTAGE OF MALE CHILDREN	SEX RATIO
Winston's Old Com. and I.	1 614	5.0	4 151	51.9	107.8
New Com. and I.	200	4.7	458	49.1	96.6
	200	4.4	492	56.0	127.1

* Four or more children.

The difference of 6.8 in the percentage of male offspring between the Old and New Com. and I. data of large families is four times its standard error and therefore highly significant. However, the average family size of the two groups of large families is not quite equal, and we therefore decided to compare four-child families of the old and new sets by themselves. As there were not enough four-

child families in the set of large families, we added to them the remaining four-child families from the Old and New Com. and I. data; the results of this comparison are shown in Table 4.

The difference of 6.1 in the percentage of males between the two sets of Com. and I. data is 2.2 times its standard error and therefore significant. Thus we may conclude that, in the course of the twentieth century, the improvements in living conditions counteracted, in the

TABLE 4

The sex ratio of four-child families of upper class Americans

DATA	NUMBER OF FAMILIES	NUMBER OF MALE BIRTHS	PERCENTAGE MALE BIRTHS	SEX RATIO
Winston's	820	1 720	52.4	110.3
Combined Old Com. and I.	146	289	49.5	98.0
Combined New Com. and I.	165	369	55.6	125.3

upper social strata, the unfavorable effect that repeated childbirth is supposed to have on the secondary sex ratio (see ref. (3)). We are further led to assume that, after the first World War, large families of the upper social strata were in the majority planned to be large, and that women who were not strong and suffered during pregnancy would not plan large families and thus cut down on miscarriages. This would help explain why the percentage of male births in large families of the Com. and I. data (Table 3) is 56 per cent as against "only" 53 per cent for families of all sizes (Table 1) and help explain also why Winston found the highest sex ratio in his two-child rather than one-child families. Miscarriages keep down the size of a family.

As a further check on the claim of earlier investigators (3) that the percentage of male births decreases with each successive pregnancy, we computed and listed in Table 5 the percentage of male births by order of birth.

The breakdown of the data as given in Table 5 offers further proof for our contention that birth order has no effect on the sex ratio of present day families of the upper social strata. In the two sets of

older data, there is a sharp drop in the sex ratio of the second birth, but the sex ratio goes up again with the third child. If the birth dates of the younger children of the two sets of old data overlap chronologically with the birth dates of the older children of the two sets of new data, we may have a logical explanation as to why the sex ratio in the two older sets of data went up again for the third and following child after first dropping for the second child. However, this hypothesis could not be verified on data from *Who is Who in Commerce and Industry* since no birth dates are given for the children.

TABLE 5

Birth order and the sex ratio of upper class Americans
(Figures in parentheses indicate number of all children.)

DATA	PERCENTAGE OF MALE BIRTHS			
	First birth	Second birth	Third birth	Fourth and later births
		Old data		
Com. and I.	53.2 (600)	49.4 (502)	53.5 (316)	50.0 (206)
Large families	46.0 (200)	41.5 (200)	55.5 (200)	52.4 (332)
		New data		
Com. and I.	54.0 (618)	54.1 (505)	55.6 (261)	45.9 (157)
Large families	59.0 (200)	57.5 (200)	57.0 (200)	51.6 (281)

No other listing of white Americans of the *Who's Who* type does give birth dates for the children, but they are listed in *Wer Ists* (7), the German equivalent of *Who's Who*. From it we collected two sets of data: (a) all 880 families with one or more children born in Germany between 1915 and 1918 inclusive, *i. e.* during World War I, and (b) a sample of 800 families, the children born either before or after World War I.

Besides giving us a chance to test within a single family the hypothesis that improved living conditions and medical care may offset

the unfavorable effect (if any) that repeated childbearing may have on the secondary sex ratio, these German data also give us an opportunity to investigate whether during World War I the sex ratio rose within a single family as it did for Germany as a whole. J. Huxley (8) has made the suggestion that the conspicuous rise in the secondary sex ratio during the later war years in Belgium, Britain, France and Germany may have been brought about by "war conditions—whether nutritional or, more probably, dependent on nerve strain—acting through the sympathetic nervous system on the organism, converting some zygotes which in normal times would have been female into male."

The children of the set of war data fall into three groups, depending on whether they were born before, during, or after World War I, and the sex ratio for each group was computed and listed in Table 6.

TABLE 6

Effect of World War I on the sex ratio of upper class German families

CHILDREN BORN	NUMBER OF CHILDREN	SEX RATIO	PERCENTAGE OF FIRST-BORN IN DATA
1900-14 (Before War)	995	107.4	52.9
1915-18 (During War)	1 080	115.1	32.8
1919-34 (After War)	700	125.1	None

The sex ratios for the three groups of children listed in Table 6 allow us to conclude that those living conditions which improved consistently for the expectant mother from 1900 through 1935 caused an increase in the sex ratio within even the single family, *i. e.* regardless of birth order, a trend which was not disturbed in either way by the war.

The first World War did bring about a considerable decrease in the number of births in Germany; the number of births per 1,000 population decreased from 26.8 in 1914 to 13.9 in 1917, rising back to 25.8 by 1920. We must remember that the average German of the lower social strata had little chance to escape induction into the Imperial Army, and his chances of becoming an officer were negligible. As an enlisted man he received home leaves very much less frequently than the overprivileged German officer and therefore had much less

chance of becoming a father during the war period. An examination of the wartime occupations of the 880 fathers of the German war data revealed that more than half of the fathers were not drafted into the army, but were exempted because they held important war positions in war industry, in government administration, military and civilian hospitals, and so on; about a fifth were senior officers during the war, and about a tenth were first officers at the front and then held important civilian positions back home. None of the fathers was an enlisted man during the *entire* war period.

It thus seems likely that the decrease in the number of births in Germany during World War I meant mainly a decrease in the number of births in the lower social strata, so that there was an increase in the relative proportion of births from the upper social strata, bringing with it the higher wartime sex ratio for Germany as a whole. As a matter of fact, the 880 men of the German *Who's Who* to whom children were born during the war period, themselves appear to be a select group from within the total group of upper class families listed. For in the sample of 800 families with children born in Germany before and/or after World War I the sex ratio was found to be "only" 106 for prewar and 114 for postwar children.

THEORETICAL CONSIDERATIONS

The main theory advanced in this paper is that improvement in living conditions in the last thirty years has caused an increase in the secondary sex ratio in the upper social strata. However, it might be argued that the biographical data used by the author in arriving at this conclusion are not reliable enough to allow the present study, *i. e.* it might be argued that there exists a strong tendency on part of the fathers to underreport their female offspring, and that this is the main cause for the high sex ratio reported by both Winston (1) and this author for families from the upper social strata. However, such an underreporting of female offspring will cause a rise in the secondary sex ratio *within* a single family (see Table 6) only if the fathers underreport their younger daughters only. This seems incredible.

Also it is possible to show by statistical means that there has been no significant omission of female offspring in the data of the present study. If p is the proportion of male offspring in the sample, and if there are N_k families with k children, we should expect $N_k p^k$ families with only male offspring. Any significant excess of "sons only" families

could be attributed to an underreporting of female offspring. Since Winston (9) has shown that the desire for male offspring causes a significantly higher sex ratio in the last-born child in families practising birth control, the size of a family is not independent of the sex ratio of the older children. For this reason, one- to four-child families were combined in this study, so that the theoretical frequency of "sons only" families is given by the formula: $N_1p + N_2p^2 + N_3p^3 + N_4p^4$. Confining this test to the two sets of our data with the highest sex ratio, we obtain the following results:

DATA	NUMBER OF "SONS ONLY" FAMILIES	
	Expected	Observed
New Com. and I.	149.9 ± 10.4	149
German War	161.6 ± 11.3	152

We may conclude that there has been no significant underreporting of female offspring in our data.

This author agrees with Winston (1) that lower-than-average intrauterine mortality is the main factor in the higher secondary sex ratio of the upper social strata, and for the following two reasons: (1) It best explains the rise in the secondary sex ratio observed in single families (Table 6), and (2) it agrees with results obtained in animal experimentation. Parkes (12) has found that the secondary sex ratio in mice would fall from 100 to as low as sixty when he artificially increased the abortion rates by unfavorable environmental conditions.

In an anatomical study on 1205 stillbirths, E. L. Potter (10) determined the causes of prenatal deaths and classified them into two groups depending on whether the abnormal condition was primarily one of the maternal organism (premature labor, asphyxia, syphilis, etc.) or one of the fetus (*i. e.* malformations). As Dr. Potter points out, malformations are unpreventable, and it is interesting to note that the autopsies revealed a larger percentage of females than males among the malformed fetuses. In other words, the sex ratio of miscarriages which could have been prevented is above the sex ratio of all stillbirths, which was about 135 for Dr. Potter's data. (These data include, however, some colored babies.)

Assuming a sex ratio of about 145 among fetuses aborted because of a preventable disorder, and assuming the percentage of preventable

miscarriages to be about 13 per cent of all pregnancies (Parkes (11)), a complete abolishing of all preventable miscarriages would raise the secondary sex ratio for the general population from 105.5 to about 110 or 111. It may be that the still higher sex ratio of 137 observed by Woods (2), and 125 observed by this author, has a genetic foundation. In order to account for a primary sex ratio different from 100, geneticists have assumed a greater motility for the Y sperm, especially since in fowl, where the XY genotype is a female, more females than males are born. It may be that this differential in sperm motility is greater in healthier males. And it may also be that the incidence of sex-linked lethals is less among the more intelligent.

One last point remains to be cleared up. Though birth control does make it possible for a family to stop reproduction after the desired number of male offspring have been born, such planned termination of reproduction will not, as has been pointed out by W. N. Norton (13), raise the sex ratio of the group practising birth control. The effect of birth control on the sex ratio is more indirect. J. Yerushalmy (14) has shown on more than 7,000 cases that relatively short and long intervals between successive births are associated with higher stillbirth rates than moderate intervals. Since the stillbirth rates are lower than average for women in the upper social strata, it appears that planning optimum intervals between successive births with the help of birth control, reduces the number of stillbirths and raises the sex ratio of the group practising birth control.

SUMMARY AND CONCLUSIONS

Problem. On a stratified sample of 3,898 families belonging to the American and German upper social strata, statistical investigations were made as to the effect of birth order, family size, a major war, and improved living conditions on the secondary sex ratio of man.

Results. (1) The author confirms the findings of S. Winston (1) that (a) the sex ratio of upper class families is significantly above average, and (b) as the size of the families of men married before 1910 increases, the sex ratio of the offspring approaches the sex ratio of the total population.

(2) However, in the families of men married since 1918, any effect of family size and birth order on the sex ratio was found to have disappeared. There is, thus, a continuous rise in the sex ratio of upper class families in the last thirty years, a rise more pronounced

in large families, so that today the sex ratio of both small and large upper class families in America and Germany is about 125 or 120 males for every 100 females.

(3) The children of a group of 880 upper class German families fell into three groups according as to whether they were born before, during, or after World War I, and there was a steady rise with time in the sex ratio of the three groups. The 1,080 children born in Germany during World War I had a sex ratio higher than that of their prewar and lower than that of their postwar siblings. The author attributes the conspicuous wartime rise and postwar drop in the sex ratio of Germany at least partly to a wartime increase in the proportion of children from the upper social classes where the men had a better chance of remaining civilians because of their professions, and a better chance of becoming high-ranking officers with frequent army leaves.

(4) It was pointed out that birth control may have only an indirect influence on the secondary sex ratio. Substitution of birth control for induced abortion when pregnancy is not desired and employment of birth control for the sake of spacing pregnancies will cut down miscarriages and stillbirths and thus increase the secondary sex ratio, since miscarriages and stillbirths are much more often males than females.

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FACTORIAL ANALYSIS OF A MULTIPLE DICHOTOMY*

BY CHARLES P. WINSOR

IN a recent paper Lombard and Doering (1) discussed the problem of significance testing in multiply dichotomized contingency tables. They did not discuss directly the question of estimating the effects whose significance is being tested. It is the purpose of the present note to indicate a method of estimation which may occasionally be useful. The method, where applicable, provides the standard errors of the estimates, together with significance tests for effects which have been assumed non-existent. The method proposed is essentially that suggested by Yates (6, p. 65).

It will be convenient to give the discussion in terms of a concrete example, and it would be hard to find a better than that used by Lombard and Doering. Table 1 is taken from their paper. The data consisted originally of scores on a test for information about cancer; these were dichotomized as "Good" or "Poor," and subdivided by all combinations of presence or absence of the four independent variables, "Newspapers," "Solid Reading," "Radio," and "Lectures." Thus, for example, there were a total of 169 persons tested who had obtained information from newspapers, radio, and solid reading, but not from lectures; and of these, 102 had scores classed as "Good" and 67, scores classed as "Poor." The data thus form a 2^5 contingency table, or may equally be regarded as a 2^4 factorial experiment on the effects of the presence or absence of four independent variables on the proportion of good scores.

It is this latter point of view which we adopt. The statistical analysis of such experiments has been extensively treated by Fisher (2), Yates (3), and others. The present data, however, offer certain complications. We note, first, that the numbers of observations vary widely from one combination of independent variables to another, with con-

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sequent variation in precision. Second, there is no reason to expect that the effects of the independent variables will be most simply expressed in terms of the proportion of good scores.

If we could assume that the effects of the independent variables were additive we should plainly have as simple a scheme as could

TABLE 1

Good and poor scores on cancer information test, by sources of information

G = Good score; P = Poor score; T = Total.

	NEWSPAPERS											
	Radio						No radio					
	Solid reading			No solid reading			Solid reading			No solid reading		
	G	P	T	G	P	T	G	P	T	G	P	T
Lectures	23	8	31	8	4	12	27	18	45	7	6	13
No lectures	102	67	169	35	59	94	201	177	378	75	156	231

	NO NEWSPAPERS											
	Radio						No radio					
	Solid reading			No solid reading			Solid reading			No solid reading		
	G	P	T	G	P	T	G	P	T	G	P	T
Lectures	1	3	4	4	3	7	3	8	11	2	10	12
No lectures	16	16	32	13	50	63	67	83	150	84	393	477

From Lombard and Doering (1), Table 1.

be expected. We shall consider in a moment what can be done to obtain this additivity; we first indicate the symbolism we adopt. We indicate the various combinations of the independent variables (which we shall call "treatments" for convenience) as, say, ($nRsL$), meaning "No newspapers, Radio, No solid reading, Lectures." If we assume

additivity, we may write

$$(nRsL) = m - n + r - s + l \qquad (1)$$

as the equation which should be satisfied apart from sampling variations. Note that the presence and absence of a treatment are symbolized by the addition and subtraction of the corresponding quantity. This is conventional but useful. We further remark that, as developed at length by Fisher and his school, failure of additivity may be treated by the introduction of additional terms ("interactions") on the right hand side of (1). We shall not be concerned here with these; we shall, however, obtain a significance test for them.

As we noted above, there is no reason to expect that the treatment effects will be simply additive when measured in a scale of proportions of good scores. Indeed, it is clear that this could not be generally true, for the proportion necessarily lies between zero and one. We therefore inquire whether some transformation of the scale of proportion of good scores might be found in which effects would be at least approximately additive.

Remembering that the measurements were originally test scores, we may well suppose that the test score might have been such as to give a normal distribution. It is then supposable that the effect of the presence or absence of any treatment might be to shift the distribution of scores along the axis without change of form; and without undue strain we can imagine that the position of the distribution for any particular treatment combination might be simply expressed as the sum of displacements due to each treatment separately. Fig. 1 will illustrate the conception.

We are not here given the original distribution of scores (and in many experiments it may not be possible to obtain it); but from the observed proportion of good scores we can obtain the equivalent normal deviate (or probit) corresponding, and hence the relative positions of the means of the assumed normal distributions.

It should be emphasized that we have not given a proof that this transformation has theoretical validity. We have merely found a transformation for which additivity of effects is algebraically possible, and it remains to be seen whether the data correspond. Further, there are many other transformations which might also permit additivity. Our choice of the probit transformation is based on familiarity and

the fact that convenient tables are available. (Fisher and Yates (4), Tables IX and XI, in particular.)

Table 2 gives, for each treatment combination, the proportion of good scores, the corresponding probit, and the weight of the probit.

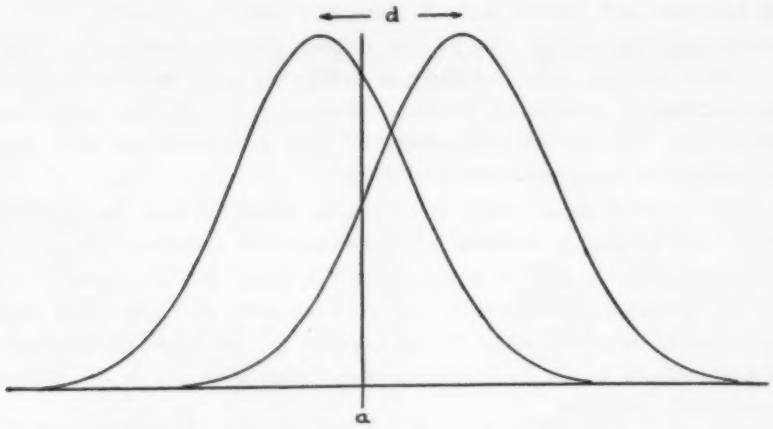

FIG. 1. SKETCH TO ILLUSTRATE CONCEPTION UNDERLYING PROBIT TRANSFORMATION
a: cut-off point (e. g., good-poor dichotomy);
d: shift in distribution associated with some difference in treatment.

We have now reduced the problem to the analysis of a 2^4 factorial experiment which differs from the usual factorial experiment only in that the individual observations are of varying precision, as indicated by the weights of Table 2. If we are willing to admit the existence of interactions of all orders (when we shall be able to reproduce the observations exactly) we need not concern ourselves about weights, and may use any convenient method for obtaining estimates of the main effects and interactions. Such methods have been given by Fisher (2), Yates (3), and others. However, this amounts to giving up the additivity of effects which we were seeking; and we have therefore to inquire how to obtain estimates of the main effects when the interactions are assumed all zero.

It is possible to attack the problem as a least squares estimation problem, with observations of varying weights. A simpler procedure is, however, possible. We note that any pair of observations for which the treatments differ only in that a particular treatment is present in

one member of the pair and absent in the other gives us, by difference, an estimate of the effect of that treatment (since interactions are assumed non-existent). Further, the weight of this difference can be found. We can thus get a weighted average estimate of any treatment

TABLE 2

Proportion of good scores, probits, and weights

TREATMENT COMBINATION	TOTAL SCORES	FRACTION GOOD/TOTAL	PROBIT	WEIGHT
NRSL	31	.742	5.650	17
NRSl	169	.592	5.233	105
NRsL	12	.667	5.432	7
NRsl	94	.372	4.673	58
NrSL	45	.600	5.253	28
NrSl	378	.532	5.080	240
NrsL	13	.538	5.095	8
Nrsl	231	.325	4.546	137
nRSL	4	.250	4.326	2
nRSl	32	.500	5.000	20
nRsL	7	.571	5.179	4
nRsl	63	.206	4.180	31
nrSL	11	.273	4.396	6
nrSl	150	.447	4.867	95
nrsL	12	.167	4.034	5
nrsl	477	.176	4.069	219

Treatment combinations: Capitals = present, lower case = absent.
N = Newspapers; R = Radio; S = Solid Reading; L = Lectures.
Probit values from Table IX, weights from Table XI, Fisher and Yates (4).

effect from the eight pairs which are obtainable for that treatment. The method of calculation is outlined in the Appendix. The calculated values of the treatment effects are given in Table 3.

It should perhaps be remarked that this form of calculation does not give all the information that the least squares solution provides; in particular it gives us no information on the covariances of the effects. In the usual factorial experiment, with all observations of equal

precision, the covariances vanish, but this is not the case here. In many situations, however, the covariances are of minor interest. If they are required, the least squares solution will provide them.

The adequacy with which the observations are described by the calculated treatment effects provides us with a significance test for the interactions. We calculate for each treatment combination the probit value expected, obtain the corresponding expected good and poor scores, and compute χ^2. We obtain $\chi^2 = 13.85$ on 11 degrees of freedom, cor-

TABLE 3

Estimates of treatment effects and standard errors

1. All main effects present: $m = 4.716 \pm .032$ $n = .197 \pm .035$ $s = .300 \pm .034$ $r = .084 \pm .038$ $l = .136 \pm .060$	Goodness of fit: $\chi^2 = 13.85$ on 11 d. f.
2. Assuming $r = 0$: $m = 4.735 \pm .032$ $n = .212 \pm .035$ $s = .307 \pm .034$ $l = .141 \pm .059$	Goodness of fit: $\chi^2 = 9.19 + 10.35 = 19.54$ d.f. $= 8 + 4 = 12$ Significance of r: $\chi^2 = 19.54 - 13.85 = 5.69$ on 1 d.f.; P $= .02$ nearly.
3. Assuming $l = 0$: $m = 4.725 \pm .032$ $n = .202 \pm .035$ $s = .306 \pm .034$ $r = .100 \pm .037$	Goodness of fit: $\chi^2 = 15.33 + 2.69 = 18.02$ d.f. $= 8 + 4 = 12$ Significance of l: $\chi^2 = 18.02 - 13.85 = 4.17$ on 1 d.f.; P $= .05$ nearly.

responding to a P of about .25, indicating that the assumption of additivity gives a satisfactory description of the data.

It is of some interest to go further, and inquire whether in fact all the independent variables are needed. From Table 3 we see that l and r are the logical candidates for omission, as judged by their ratio to their standard errors. We observe that if we assume one of them, say r, to be zero, pairs differing only in the presence or absence of r become, by hypothesis, samples from the same population. We can

therefore test the pair as a 2 x 2 table; and we shall have 8 such tables, giving a χ^2 on 8 degrees of freedom. The appropriate marginal distributions of these tables give us the material from which to obtain estimates of the remaining main effects.

The results as given in Table 3 indicate that probably neither l nor r can be taken as without effect. For each the increase in χ^2 is significant at the 5 per cent level. We note that this is in agreement with what we should expect from a comparison of the estimates with their standard errors.

It will perhaps be helpful to indicate what our analysis has led to in terms of the original problem. We may suppose that the purpose was to determine what media are most effective in disseminating information about cancer. We conclude that the order of effectiveness is (1) solid reading, (2) newspapers; and that radio and lectures are less effective than the other two. Quantitative measures of effectiveness have also been found, with their standard errors.[1]

We may remark that our method of estimation has not minimized χ^2, nor is it a maximum likelihood estimate. It can hardly differ much from either of these in the present case, and we do not believe that it is ever likely to differ much. The method is not, however, offered with any claim that it is more than a reasonably rapid method of getting reasonably good estimates.

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[1] In presenting this interpretation, I realize that I am on very dangerous ground. In particular, I do not wish to be understood as making statements about the effectiveness of various media in disseminating information in general.

APPENDIX

DETAILS OF COMPUTATION

Table 2 gives, for each of the 16 treatment combinations, the probit value corresponding to the fraction of good scores, and the weight. The latter is obtained by multiplying the weighting coefficient (from Fisher and Yates (4), Table XI) by the total scores available for this treatment combination. The weights have been rounded to the nearest integer. (Tukey (5) has a useful discussion of weighting.)

Since we are assuming that all interactions are zero, each probit value is an estimate of the quantity

$$m \pm n \pm r \pm s \pm l$$

where the signs are + or — according as the corresponding treatment is present or absent. Thus, for example, we have the estimates

$$5.650 \text{ for } m+n+r+s+l;$$
$$5.233 \text{ for } m+n+\mathrm{r}+s-l.$$

The weights of these are 17 and 105 respectively. Their difference gives us the estimate

$$\Delta = .417 \text{ for } 2\,l,$$

with the weight 14.6, obtained from $\frac{1}{17} + \frac{1}{105} = \frac{1}{w}$.

We thus obtain a set of 8 estimates of $2l$, with their weights. The weighted average of these gives us our final estimate,

$$l = \tfrac{1}{2}\,\frac{\Sigma w\Delta}{\Sigma w} = \tfrac{1}{2}\,\frac{14.6 \times .417 + \ldots}{14.6 + \ldots}.$$

The standard error, σ_l, of this estimate is obtained from

$$\sigma_l^2 = \frac{1}{4\,\Sigma w}.$$

The estimate of m is simply the weighted average of the 16 probit values in Table 2. The variance of this estimate is simply the reciprocal of the sum of the weights.

We thus obtain estimates of the various treatment effects, measured on the probit scale, together with their standard errors. We have now

to investigate whether the scheme we have assumed gives an adequate representation of the data.

For this purpose we calculate for each treatment combination the probit value from our estimates; thus

$$NRSL = 4.716 + .197 + .300 + .084 + .136$$
$$= 5.433$$

We now transform back to the proportion of good scores, obtaining .6675 as the expected proportion of good scores. Since there were 31 observations in this category, we have the comparison:

	Good	*Poor*
Observed	23	8
Expected	20.7	10.3
Diff., O - E	2.3	−2.3

with a contribution to χ^2 of

$$2.3^2 \left(\frac{1}{20.7} + \frac{1}{10.3}\right) = .77.$$

Each of the other treatment combinations will give a similar computation. The total χ^2 will have 11 degrees of freedom, since we have computed 5 estimates from the data.

If now we decide to assume one of the treatment effects nonexistent, say *L*, then such a pair as *NRSL, NRSl* are by hypothesis samples from the same population. For this pair we have:

	Good	*Poor*	*Total*
NRSL	23	8	31
NRSl	102	67	169
NRS	125	75	200

The lower margin of this table now gives us, on transformation to the probit scale, an estimate of

$$m + n + r + s.$$

Similarly, the results for *NRsL, NRsl* may be combined to give an

estimate of $m + n + r - s$; and we may proceed to obtain our estimates of treatment effects as before.

In testing goodness of fit, we remark that the eight 2 x 2 tables may each be tested by χ^2 in the usual way, with one degree of freedom each. This test is essentially a test of the existence of (in this instance) the factor L and its interactions with N, R, and S, and is independent of the values and interactions of the latter. We may also test the fit of N, R, and S on the marginal tables from which we formed our estimates. We have thus a total of 12 degrees of freedom with a corresponding χ^2. The difference between this and the χ^2 on 11 degrees of freedom when all main effects are fitted gives us a significance test for the main effect of L. We observe that in this comparison no continuity correction is to be used.

NOTES

SOME ELEMENTARY PROBLEMS OF IMPORTANCE TO SMALL SAMPLE PRACTICE*

1. INTRODUCTION

THIS memorandum outlines a few problems concerning small samples whose solution would have considerable practical application and which seem approachable by some combination of experimental sampling, empirical approximation, and, possibly, analytical investigation.

The first group of these problems is connected with the behavior of t for non-normal distributions and the possibilities of finding an improved expression for use in such cases.

The second group is related to the "Studentization," in a generalized sense, of the sample variance. These problems are generalized to the higher k-statistics.

Finally, the last group of problems is concerned with t and its modifications when the population is finite.

These problems are being circulated in the hope of encouraging work by various persons. (The investigation of the effects of non-normality by experimental sampling seems an ideal field for cooperative work. In an attempt to avoid duplication, the Statistical Research Group, Princeton University, will be glad to receive information about work in progress, and will try to make this information available to others working in the field.)

2. THE PROBLEM OF t

It has been appreciated for a long time that two-sided t-tests are insensitive to deviations from normality. That is, the percentage of the time that a null t (that is to say, a value of t calculated with the

* Prepared in connection with research sponsored by the Office of Naval Research.

assumed mean equal to the population mean) exceeds the symmetrical significance points calculated from the normal distribution is not greatly affected by the nature of the underlying distribution. In the original paper in which he introduced t, Student (7, 1908, p. 19) indicated that this might be so. This was later verified by experimental sampling, the most notable work being that of Egon Pearson (5, 1930). The fact that this was not true for one-sided t-tests has provoked considerable discussion (for example, *cf.* Karl Pearson (6, 1928)), and has recently been emphasized by Geary (2, 1947).

It would be highly desirable to have a modified version of the t-test with a greater resistance to skewness when used as a one-sided test. Since there is always a *quid pro quo* in statistics (unless inefficient methods are involved), we must expect to lose a little power when the underlying distribution is symmetrical, but this loss may be expected to be very small. A similar allowance for the kurtosis of the sample might possibly be worthwhile if very simple.

Casting about for a possible way in which to bring about this modification, our attention is struck by the work of Egon Pearson (5, 1930, pp. 280-286) and Walsh (unpublished) on the use of the expression

$$\frac{\text{midrange} - \text{assumed mean}}{\text{range}}$$

in very small samples. The efficiency is very high when the underlying distribution is normal, and the distribution is remarkably little affected by deviations from normality. It thus seems possible that an improved modification of the t-test might

(1) be based on:

the sample mean,
the sample variance,
the sample size,
the upper and lower sample semiranges,
(upper semirange = greatest observation minus sample mean
lower semirange = sample mean minus smallest observation);

(2) approximately reduce to a constant times the "midrange/range" expression in samples of 3 and 4 (samples of 2 are trivial); and

(3) approximately reduce to t for large samples without an unusually discrepant observation.

An example of an expression with roughly plausible properties is

$$t + K \frac{\text{upper semirange} - \text{lower semirange}}{\text{upper semirange} + \text{lower semirange}} t^2$$

where

$$t = \frac{\text{sample mean} - \text{assumed mean}}{\sqrt{\frac{\text{sample variance}}{\text{sample size}}}}$$

and K may depend on the sample size. Whether this is the right sort of expression is, of course, doubtful.

Further clues can probably be found by studying that extremely non-normal distribution in which all the probability is concentrated at two points. The means of random samples from this distribution of course follow the binomial distribution. We are all aware that for moderate probabilities the binomial distribution is nearly normal (and that the semiranges are nearly equal). But for very small or very large probabilities the binomial distribution is far from normal until the sample size is very large (and the semiranges are very different). Since good tables of the binomial distribution exist, it is perhaps the most natural non-normal distribution with which to *begin*.

If x cases occur out of n, then the sum of squares of deviations is $x - (x^2/n)$. If P is the assumed value of the probability, then

$$t = \sqrt{n-1}\,\frac{p - P}{\sqrt{pq}}, \text{ where } p = \frac{x}{n},$$

and, unless all observations fall in one class,

$$\text{upper semirange} = 1 - p,$$
$$\text{lower semirange} = p,$$

so that all relevant quantities are very easily computed.

3. SPECIFIC PROBLEMS

With this background we can now formulate some specific problems.

Problem A1

What modifications of t are (i) simple, (ii) distributed with less dependence on the underlying distribution than t, and (iii) have reasonable power?

How are these modifications distributed when the underlying distribution is normal?

Problem A2

What modifications of t are (i) simple, and (ii) distributed similarly for normal and binomial underlying distributions?

How are these modifications distributed when the underlying distribution is normal?

Problem A3

Are there general procedures for finding such modifications of given expressions, which are less dependent on the underlying distribution?

4. THE PROBLEM OF THE VARIANCE

Many years ago Student realized the value of expressing the difference between observed and assumed means in terms of the observed standard deviation. This had finally become natural, for Student was working with problems where he was convinced that he did not know the scale of the population, — the sample standard deviation had to be used to estimate the population standard deviation, and this was clearly fallible. With the aid of this principle we have gone very, very far in dealing with all sorts of problems involving means. The division of some expression of linear dimensions by an independent estimate of a population standard deviation has indeed been honored by the name "Studentization" and has been tremendously useful.

It seems to be time to contemplate Studentization from a different point of view. The essential elements seem to be the following:

(1) Something to be used for inference, whose variability depends on the situation in the model (in the simplest case — the variance of the population);

(2) Something else to be used to provide evidence for the situation in the model; and

(3) A way of combining these into an expression whose variability is reasonably independent of the situation in the model.

It seems to me that at the present state of development of statistical inference it is time to suggest rather strongly that all statistical problems of estimation rather than decision should be Studentized to the greatest

extent possible. That is, the model should involve as many "nuisance parameters" as can be compensated for. (The maximization of considered alternatives has already appeared in connection with the problem of modifying t.) No problem without "nuisance parameters" should be considered in satisfactory shape, since it does not allow for the deviations from simple hypotheses which will occur in practice.

Let us now apply the principle to the variance. The classical standard frame of reference, the classical standard model, is the set of all normal distributions. The variability of the sample mean is not fixed within this model, since it depends on the population variance. On the other hand, the variability of the *logarithm of the sample variance* (or equivalently, of the logarithm of the sample standard deviation) is fixed in this model since the distribution of

(log sample variance) — (log population variance)

is that of the logarithm of chi-square (with degrees of freedom equal to the sample size). Thus to get what seems to be a desirable situation we must generalize the model. A plausible way to generalize the model which will lead to exact solutions does not seem to be known to date. In the absence of such a proposal, it seems worthwhile to attack the situation in the opposite way.

5. THE PLAUSIBLE RATIO

For reasons which will be discussed below it seems natural to consider the following expression as one plausible analog of Student's t for the variance:

$$\frac{\log k_2 - \log K_2}{\sqrt{2 + \frac{k_4}{k_2^2}}}$$

where

$$k_2 = \frac{1}{n-1}\Sigma\,(x_i - \bar{x})^2 = \text{sample variance},$$

$$K_2 = \text{assumed population variance},$$

$$k_4 = \frac{n}{(n-1)\,(n-2)\,(n-3)}\Big\{\,(n+1)\,\Sigma\,(x_i - \bar{x})^4 - 3\,(n-1)\,\Sigma\,(x_i - \bar{x})^2\Big\}.$$

Let us recall that k_4 is the fourth of Fisher's k-statistics, and is the unique symmetric function of the observations whose expectation is the fourth seminvariant (i. e., the fourth cumulant). The moments of k_4/k_2^2 for an underlying normal distribution have been discussed by Fisher (1, 1930), and he has pointed out that k_2 and k_4/k_2^2 are independently distributed when the underlying distribution is normal.

The fact that $\log k_2 - \log K_2$ has a fixed distribution when the underlying distribution is normal and K_2 equals the population value suggests that we should try this over some denominator. For large samples we want,

$$(\text{denominator})^2 \doteq \text{var}(\log k_2) \doteq \kappa_2^{-2}\,\text{var}(k_2),$$

whence

$$\text{denominator} \doteq \frac{\sqrt{\text{var}\,(k_2)}}{\kappa_2} = \frac{\sqrt{\kappa_4 + 2\kappa_2^2}}{\kappa_2} = \sqrt{2 + \frac{\kappa_4}{\kappa_2^2}},$$

which suggests the use of the proposed denominator.

This very large sample argument receives further confirmation from the work of LeRoux (4, 1931, pp. 152-3) who found that a certain particular one-parameter family of Pearson curves, which he calls $D(s^2)$III populations, gives distributions of sample variances whose first four moments are closely those of multiples of chi-square on modified degrees of freedom. This holds with good accuracy for samples of 10 and up. The apparent number of degrees of freedom, n^*, is found, in this case, from

$$\frac{n}{\beta_2 - \dfrac{n-3}{n-1}} - 1 = \frac{n^* - 3}{2},$$

which can be written

$$n^* - 1 = \frac{n-1}{1 + \dfrac{\kappa_4}{2\kappa_2^2}\left(1 - \dfrac{1}{n}\right)}.$$

The two principal terms in the denominator are thus allowed for the proposed fraction.

6. SPECIFIC PROBLEMS

With this background we can now state some specific problems.

Problem A4

For what generalized model or models (including all normal distributions) does there exist an expression whose distribution is

independent of the situation *within* the model and which does for the variance what t does for the sample mean?

Problem A5

What is the distribution of the expression of Section 5 when the underlying distribution is normal?

Problem A6

What is the distribution of the expression of Section 5 for particular non-normal underlying distributions?

7. CONTINUATION

We have considered: (i) the problems of improving the Studentization of t, which replaces k_1 (the sample mean) which is the only symmetrical and general unbiased estimator of the first seminvariant (population mean); and (ii) the problems of "Studentizating" k_2 (the sample variance) which is the unique symmetrical and unbiased estimator of the second seminvariant (the population variance). When these problems have been solved, it will be natural to continue and find ways of "Studentizing" statistics concerned with the third and higher seminvariants. Let us state this as a problem.

Problem A7

How can statistics closely associated with k_3, k_4, . . . , be "Studentized"?

8. STUDENTIZATION AND FINITE POPULATIONS

It has always been the custom to discuss t in terms of sampling from an infinite normal population. We are prepared, and have been prepared for a long time, to drop the hypothesis of normality, and study the sampling distribution of t from infinite non-normal populations. It is also reasonable to drop the hypothesis of infinity, and study the sampling distribution of a suitably modified t when sampling a finite population!

It is well known that if κ_{1N}, κ_{2N} are the k-statistics for the finite population of size N, then

$$E_N(k_1) = \kappa_{1N},$$

$$E_N(k_2) = \kappa_{2N},$$

$$\mathrm{var}_N(k_1) = \left(\frac{1}{n} - \frac{1}{N}\right)\kappa_{2N},$$

where the subscript N reminds us that we are working with a finite population of size N. The natural analog of t is, therefore,

$$\frac{k_1}{\sqrt{\left(\frac{1}{n} - \frac{1}{N}\right) k_2}}$$

which can be written

$$\frac{n\bar{x}}{\sqrt{\Sigma\,(x_i - \bar{x})^2}} \cdot \frac{\sqrt{1 - \frac{1}{n}}}{\sqrt{1 - \frac{n}{N}}}.$$

Very little is known about the sampling distribution of such ratios from finite populations.

9. SPECIFIC PROBLEMS

This leads us to pose the following:

Problem A8

For what finite populations is the distribution of this ratio nearly that of Student's t?

Problem A9

Is there a better way to adapt Student's t to finite populations?

Problem A10

Once progress has been made on Problems *A1* and *A2*, how can these modifications of t be adapted to finite populations?

10. AND FOR THE VARIANCE?

Clearly we may also wish to apply similar ideas to the plausible expression of Section 5 or one of its successors. In this connection we naturally expand the variance of $\log k_2$ in a formal series, expecting that it may tell us about the scale of the center of the distribution of $\log k_2$ much better than it does about the corresponding second moment. Since, formally,

$$\log k_2 = \log \kappa_{2N} + \frac{k_2 - \kappa_{2N}}{\kappa_{2N}} + \frac{(k_2 - \kappa_{2N})^2}{2\kappa_{2N}^2} + \frac{(k_2 - \kappa_{2N})^3}{3\kappa_{2N}^3} + \dots$$

we have, to some degree of approximation,

$$\mathrm{var}_N(\log k_2) = \kappa_{2N}^{-2}\,\mathrm{var}_N(k_2) + \kappa_{2N}^{-3}\,\mu_{3,N}(k_2) + \frac{11}{12}\,\kappa_{2N}^{-4}\mu_{4,N}(k_2) + \ldots$$

where $\mu_{3,N}(k_2)$ and $\mu_{4,N}(k_2)$ represent the third and fourth moments of k_2 over the finite population of size N. The value of $\mathrm{var}_N(k_2)$ is known to be

$$\frac{2}{n-1}\left(1-\frac{n+1}{N+1}\right)\left\{\kappa_{2N}^2 + \left(1-\frac{1}{n}-\frac{1}{N}-\frac{1}{nN}\right)\frac{\kappa_{4N}}{2}\right\}.$$

(*cf.*, for example, Irwin and Kendall (3, 1943, pp. 140-141), but $\mu_{3,N}(k_2)$, $\mu_{4,N}(k_2)$, and so on, become more complex. The methods of Irwin and Kendall, however, would allow relatively easy determination of their leading terms. Neglecting all these higher terms, we have

$$\mathrm{var}_N(\log k_2) \doteq \frac{2}{n-1}\left(1-\frac{n+1}{N+1}\right)\left\{1+\left(1-\frac{1}{n}\right)\left(1-\frac{1}{N}\right)\frac{\kappa_{4N}}{2\kappa_{2N}^2}\right.$$

where we have neglected $2/nN$ in comparison to unity. This suggests the modified plausible ratio might be

$$\frac{\log k_2 - \log K_2}{\sqrt{\left(1-\frac{n+1}{N+1}\right)\left[2+\left(1-\frac{1}{n}\right)\left(1-\frac{1}{N}\right)\frac{k_4}{k_2^2}\right]}}$$

where the factors $(1-\frac{1}{n})$ and $(1-\frac{1}{N})$ might very well not be there (or be squared or what have you!).

II. SPECIFIC PROBLEMS

Thus we might like to know, if we find the plausible ratio well-behaved for infinite populations, the answers to such questions as:

Problem A11

Is the plausible ratio with

$$(\text{denominator})^2 = \left(1-\frac{n+1}{N+1}\right)\left(2+\frac{k_4}{k_2^2}\right)$$

distributed reasonably constantly for finite N?

Problem A12

Do the additional factors $(1 - \frac{1}{n})$ $(1 - \frac{1}{N})$ improve the situation?

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INDEX TO VOLUME XX

Abortions, sex ratio of, 156
Age changes in pituitary fossa, 7
distribution of Parsi, 79
ALLEN, IRENE, Facial Growth in Children of Five to Eight Years of Age, 109-145
ASDELL, S. A., The Relation between Inbreeding and Intelligence, 171-181
Aspects of Parsi demography, 47
Autocorrelation approach to the analysis of the incidence of communicable diseases, 90

BERNSTEIN, MARIANNE E., Recent Changes in the Secondary Sex Ratio of the Upper Social Strata, 182-194
Birth control in Puerto Rico, 21
rates, Parsi, 56
Births, by number of births to mother, and age of mother, Puerto Rico, 24

CHASSAN, JACOB B., The Autocorrelation Approach to the Analysis of the Incidence of Communicable Diseases, 90-108
Cicatrix of the neck associated with anomalous dental occlusion in one of a pair of monozygous twins, 36
Coefficient of inbreeding, and intelligence, 175
Communicable disease incidence, method of analysis, 90
Cultural aspects of birth control in Puerto Rico, 21

Death rates, Parsi, 56
Demography of the Parsi, 49
Dental occlusion associated with cicatrix of the neck, 36
Dentition and facial growth, 109
Dermatoglyphics of Mongoloid imbeciles, 146
Dichotomy, multiple, factorial analysis of, 195

Elementary problems of importance to small sample practice, 205

Facial growth in children of five to eight years of age, 109
Factorial analysis of a multiple dichotomy, 195
Fetal skulls, measurements of pituitary fossa, 3
Four-fold tables, 161
FRANCIS, CARL C., Growth of the Human Pituitary Fossa, 1-20

Gestation month, sex ratio of abortions by, 157
Growth, facial, in children, 109
of the human pituitary fossa, 1

Inbreeding and intelligence, 171

KING, MARGUERITE N., Cultural Aspects of Birth Control in Puerto Rico, 21-35

LASKER, GABRIEL W., and REYNOLDS, H. H., A Cicatrix of the Neck Associated with Anomalous Dental Occlusion in One of a Pair of Monozygous Twins, 36-46

Life tables, Parsi in Bombay, 74
"Listerism," 161

Mongoloid imbeciles, dermatoglyphics of, 146
Monozygous twins, dental malocclusion in a pair, 36
Multiple dichotomy, factorial analysis of, 195

Palmar dermatoglyphics of Mongoloid imbeciles, a study of, 146
Parsi demography, 49
Pituitary fossa, growth, 1
  area in children, 122
Population, control of, in Puerto Rico, 21
  Parsi in India, 49
"Probability and Listerism," 161
Puerto Rico, birth control in, 21

Recent changes in the secondary sex ratio of the upper social strata, 182
Relation between inbreeding and intelligence, 171
Respiratory admission rates, U. S. Army, by month, correlated, 90
Reynolds, H. H., *see* LASKER, GABRIEL W.
Royal pedigrees, analyzed for relationship between intelligence and inbreeding, 173

SEKAR, C. CHANDRA, Some Aspects of Parsi Demography, 47-89
Sex differences in facial growth, 109
  ratio, and birth order, 187
    effect of family size on, 185
    effect of World War I on, 188
    German families, 188
    in upper social strata, 182
    of abortions, a note on, 156
Small sample problems, unsolved, 205
SNEDEKER, DORIS MANN, A Study of the Palmar Dermatoglyphics of Mongoloid Imbeciles, 146-155
"Studentization," 205

t-test, 205
TIETZE, CHRISTOPHER, A Note on the Sex Ratio of Abortions, 156-160
TUKEY, JOHN W., Some Elementary Problems of Importance to Small Sample Practice, 205-214

Variance, sample, 205

WINSOR, CHARLES P., Factorial Analysis of a Multiple Dichotomy, 195-204
  "Probability and Listerism," 161-169

HUMAN BIOLOGY

a record of research

INDEX TO VOLUMES 1-20 (1929-1948)

Published 1949
THE JOHNS HOPKINS PRESS
Baltimore, Maryland
U. S. A.

Full titles and inclusive page references will be found under the author entry. Asterisked subject entries are titles, in some cases abbreviated.

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INDEX TO VOLUMES I-XX

ABERLE, S. D., WATKINS, J. H., and PITNEY, E. H. The Vital History of San Juan Pueblo, *12*, 141-188
Abnützungsquote of Rubner, *10*, 191
Aboriginal populations, demography of, *12*, 481; *19*, 83
Abortion(s), gestation month of, *13*, 168
* human, *6*, 271
in Negroes, *3*, 233
sex ratio of, *3*, 572; *10*, 53, 235; *20*, 156
*Abridged life tables for the white population of the United States in 1830, *14*, 352
Abstracts of food and nutrition research, *14*, 532
Acceleration of growth in adolescence, *7*, 330; *15*, 14
Accidents, death rate and weight, *2*, 181
frequency distribution of, *4*, 156
Acuity, auditory, *11*, 259
of vision, of Army inductees, *16*, 6
*Adaptation to exercise of Negro and white sharecroppers in comparison with Northern whites, *13*, 139
Adiposis and heart disease, *8*, 78
Adolescent acceleration of growth, *7*, 330; *15*, 14
Adrenal weight in mental disease, *7*, 369
Age, and blood pressure, *2*, 264; *4*, 518; *5*, 566
and coitus rate, *12*, 550
* and sex distributions of patients at the Mayo Clinic, *2*, 185
and specific gravity, *5*, 663
at death, of mental patients, *7*, 354; *11*, 514
at marriage, and neuroticism, *16*, 21
and physical appearance, *16*, 17
trend of, in Washington County, Md., *12*, 59
at onset of disease, variation in, with type of inheritance, *4*, 72
changes in density of hair, *3*, 314
in pigmentation of Negroes, *1*, 334
in pituitary fossa, *20*, 1
distribution, Chinese population, *19*, 3
correlation with growth-rate of population, *3*, 194
deaths in aboriginal and American Indian populations, *19*, 86
Iowa female population, *7*, 356
logistic population, *3*, 470, 484
Parsi, *20*, 79
personality disorders, *11*, 487
urban-rural, in Mexico in 1793, *14*, 506
influence on relation of IQ and physical measurements, *14*, 527
menarcheal, *see* Menarcheal age
of family head, and permanence of residence, *19*, 107
of father and survival of offspring, *11*, 342

(Age)
of mother, and epilepsy of offspring, *18*, 49
birth order, and socio-economic status, *13*, 64
stillbirths rates by, *17*, 73, 95
Age-specific death rates actually experienced by a cohort, *13*, 444
fertility rates, for generations, *19*, 133
Agglutinogens M and N, heredity of, *7*, 222
Aging and appetites, *1*, 565
Alcohol and blood pressure, *5*, 573
and longevity, *6*, 125
metabolism of, at high altitudes, *8*, 386
Alcoholic psychoses, in Negroes, *7*, 496
Alimentary tract in old age, deaths assignable to, *4*, 92, 106
*Alleged lethal effects of sexual continence in the Holy Land, *3*, 577
ALLEN, IRENE, Facial Growth in Children of Five to Eight Years of Age, *20*, 109-145
Allometry in anthropoid apes, *11*, 379
in monkeys, *13*, 302; *19*, 53
Alpha waves, growth equation for frequency of, *10*, 145
ALVAREZ, WALTER C., and ASCANIO, HUGO, Age and Sex Distributions of Patients at Mayo Clinic, *2*, 185-198
Alvarez, Walter C., *see also* HOSOI, KIYOSHI
Alveolar arches, development of, *10*, 327
*Amalgamation of ethnic groups, *7*, 437
Amenorrhea, temporary, *6*, 444
American cities, infant mortality in, *9*, 140
Indians, *see* Indians, American
* notables, ecology of, *1*, 544
Amplitude of joint movement, measurement of, *9*, 199
Analysis of personality, *1*, 131
* of the conception of race and a new method of distinguishing races, *14*, 372
*Anatomic lag, *6*, 524
* * in head and body dimensions, *13*, 473
Anatomical expedition to Africa, *1*, 423-425
Ancestors of longevous persons, *6*, 98
of stutterers, *11*, 394
tables of, in genetic research, *2*, 252
*Ancestral longevity and the sex ratio of the descendants, *12*, 403
ANDERSON, D. DRYSDALE, Point of Population Saturation, Its Transgression in Mauritius, *1*, 528-543
Andreew's index, *3*, 423
ANDREWS, J. M., A Nomogram for Computation of Probable Errors of Differences, *11*, 543-546
Anemia, sickle cell, habitus of patients with, *16*, 99
ANGEL, J. LAWRENCE, Greek Teeth: Ancient and Modern, *16*, 283-297
Race, Type, and Ethnic Group in Ancient Greece, *18*, 1-32
Angular momentum of arms, *11*, 529
Animal rhythm, seasonal, *13*, 534

Anlagen, of human foot, protrusion from body stem, *16*, 213
*Anomalies, congenital, *1*, 278
* developmental, and primogeniture, *1*, 382
*Anomalous human sex-linked inheritance of color-blindness in relation to attached sex chomosomes, *5*, 130
*Antero-posterior movements of the teeth between two and ten years, *8*, 161
Anthologia Anthropologica, *10*, 576
Anthropogeny and polyisomerism, *6*, 632
*Anthropological approach to ethnogenics, a new perspective, *4*, 239
measurements, in psychotic patients, factorial analysis of, *18*, 133
IQ, and age, *14*, 527
Anthropometric standard, for use in determining habitus type, *14*, 216
* standards for working women, *6*, 472
* studies from McCormick Fund, *10*, 285
* of individual growth. I. Age, height, and rate of growth in height, elementary school children, *7*, 319
* study of the upper extremities of children, *11*, 276
*Anthropometry, in the new-born (Body build in infants, V), *6*, 612
methods in constitution study, *1*, 37
of boys, *9*, 568
of cardiacs and non-cardiacs, *6*, 655; *8*, 49
of infants, *8*, 433
of Japanese women, *14*, 285
of Negroes, *15*, 127
of patients with sickle-cell anemia, *16*, 99
of schizophrenics, *14*, 28, 192
* of the Western and Copper Eskimos, *5*, 313
* some uses of the standard deviation in, *8*, 592
Anticipation in onset of inherited disease, *4*, 77, 554
Ape-man, is he a myth? *1*, 4, 153
Aphasia as reversion to primitive behavior, *1*, 418
*Apical dystrophy: an inherited defect of hands and feet, *4*, 179
Apoplexy, and weight, *2*, 171
in Negroes, *3*, 227
Appearance, personal, neuroticism, and age at marriage, *16*, 15
Appel, E. M., *see* APPEL, F. W.
APPEL, F. W., and APPEL, E. M., Intracranial Variation in the Weight of the Human Brain, *14*, 48-68, 235-250
Appendages, measurements of, in Negroes, *15*, 129
Appendicitis, and weight, *2*, 175
Appetite and aging, *1*, 565
* levels of food consumption: a technique for measuring foods in terms of psychological and nutritional values combined, *15*, 199
*Application of the gene-frequency method of analysis to sex-influenced factors with especial reference to baldness, *7*, 608
Arab population growth in Palestine, *9*, 353
Arboreal life and primate evolution, *2*, 225

Argentina, population, *12*, 366
Arm girth, growth in, *9*, 373
muscles, activity curve for, *11*, 534
Army officers, biometry of, *4*, 509; *5*, 61
Arteriosclerosis, cerebral, in Negroes, *7*, 488
Ascanio, Hugo, *see* ALVAREZ, WALTER C.
ASDELL, S. A., The Relation between Inbreeding and Intelligence, *20*, 171-181
ASHLEY-MONTAGU, M. F., The Dentition of Identical Twins with Particular Reference to an Identical Pathological Condition, *5*, 629-645
and TREVOR, J. C., A New Sliding Caliper for Cranial Measurements, including Internal Diameters, *9*, 417-418
Ashley-Montagu, M. F., *see also* MONTAGU, M. F. ASHLEY
*Aspects of Parsi demography, *20*, 47
Assembling of records in constitution study, *1*, 52
Association factor in stuttering, *11*, 396
theory of synesthesia, *5*, 156, 191
Asymmetry, and metopism, *17*, 131
and wormian bones, *18*, 76
in primates, *9*, 306
in skeletons of primates, *2*, 375
of gorilla, *16*, 40
of body, *11*, 447
of vertebral column, *16*, 153
reversal, in twins, *12*, 22
Ateles, relative growth, *19*, 53
Atkinson, William B., *see* DUPERTUIS, C. W.
*Auditory acuity, resemblance among siblings, *11*, 260
Australasia, migration into, *2*, 45
Australia, aborigines, demography of, *12*, 481
cancer death rates, *15*, 74
*Autocorrelation approach to the analysis of the incidence of communicable diseases, *20*, 90
Autonomic nervous system, correlational analysis of, *14*, 69
Autopsy of Thomas Parr, *3*, 138
*Autosomal linkage between the factors for the production of identical and of fraternal twinning, *11*, 179
Axillary hair, development of, *2*, 486

Badger, George F., *see* RIDER, ROWLAND
Baghdad, blood groups in, *13*, 398
BAKWIN, HARRY, Sex Factor in Infant Mortality, *1*, 90-116
The Negro Infant, *4*, 1-33
and BAKWIN, RUTH MORRIS, Body Build in Infants. V. Anthropometry in the New-born, *6*, 612-626; IX. Body Build in Eczema, *11*, 269-276
Bakwin, Ruth Morris, *see* BAKWIN, HARRY
Baldness, inheritance of, *7*, 608
Bantu, parathyroid glands of, *9*, 246

BARKER, ROGER G. and STONE, CALVIN P., Physical Development in Relation to Menarcheal Age in University Women, *8*, 198-222
Barker, Roger G., *see also* STONE, CALVIN P.
BARNES, IRENE, Inheritance of Pigmentation in the American Negro, *1*, 321-381
BARRY, HERBERT, JR., MACKINNON, DONALD W., and MURRAY, HENRY A., JR., Studies in Personality. A. Hypnotizability, *3*, 1-36
Basal chronological age and IQ relationships, *14*, 527
Basion-bregma height in American Indian crania, *14*, 179
Bavaria, restrictions on marriage in, *4*, 286
BAYER, LEONA M., and GRAY, H., Anthropometric Standards for Working Women, *6*, 472-488
Hand Size in Working Women, *8*, 267-272
BAYLEY, NANCY, Growth Changes in the Cephalic Index during the First Five Years of Life, *8*, 1-18
Bayley, Nancy, *see also* JENSS, RACHEL M.
BEALL, GEOFFREY, and STANTON, R. G., The Relationship between Epilepsy and Maternal Age, *18*, 49-60
BEARD, HOWARD H., A Biometric Study of the Relation between the Excretion of Creatinine Nitrogen and Several Body Measurements, *4*, 351-362
Coefficients of Correlation between the Nitrogenous Constituents of the Urine after Ingestion of Low, Normal, and High Protein Diets, *7*, 419-429
*Beauty, marriage and fertility, *10*, 575
BECKHAM, ALBERT SIDNEY, A Study of Weight and Stature of Negro City Children of Adolescent Age, *10*, 124-135
Bedouin dermatoglyphics, *9*, 357
BEEBE, GILBERT W., and GAMBLE, CLARENCE J., The Effect of Contraception upon Human Fertility, *10*, 372-387
and GEISLER, MURRAY A., Control of Conception in a Selected Rural Sample, *14*, 1-20
Beecher, H., *see* TODD, T. WINGATE
Behavior disorders and biophysical factors, *10*, 93
longitudinal method of study of, *9*, 542
of baby gibbon, *5*, 291
primitive, reversion to from organic brain disease, *1*, 406
sexual, of chimpanzee, *11*, 78
Behaviorism and the cerebral cortex, *2*, 461
"Behavioristic" psychology, genetic refutation of, *3*, 547
BELLOWS, ROGER M., The Psycho-genetic Relationship, *4*, 280-285
Berger rhythm, growth equation for frequency of, *10*, 145
Berkeley, California, fertility study, *7*, 543
BERKSON, JOSEPH, Evidence of a Seasonal Cycle in Human Growth, *2*, 523-538
Growth Changes in Physical Correlation, *1*, 462-502
BERNARD, JESSIE, Some Biological Factors in Personality and Marriage, *7*, 430-436
BERNSTEIN, MARIANNE E., Recent Changes in the Secondary Sex Ratio of the Upper Social Strata, *20*, 182-194
Bernstein's theory, analysis of exceptions to, *17*, 13
BERRY, MILDRED FREBURG, Twinning in Stuttering Families, *9*, 329-346

von Bertalanffy, Ludwig, A Quantitative Theory of Organic Growth (Inquiries on Growth Laws, II), *10*, 182-213
Bertalanffy's growth formula, *11*, 402
Betz, Barbara J., Somatology of the Schizophrenic Patient, *14*, 21-47, 192-234
Biacromial and biiliac diameters of different disease groups, *6*, 691; *8*, 68
diameters of Japanese women, *14*, 304
of schizophrenics, *11*, 360; *14*, 29
width, *7*, 152
Bicristal diameters, of Japanese women, *14*, 305
of schizophrenics, *11*, 360; *14*, 29
Biiliac diameters of pubescent and non-pubescent girls, *9*, 27
Bile passages, diseases of, and sex, *2*, 73
Bioclimatology and biometeorology in Biological Abstracts, *11*, 548
Biological classification of causes of death, *3*, 496
constitution, correlated with mental disease, *14*, 23
* factors in Negro mortality, *1*, 229
* in personality and marriage, *7*, 430
in public health, *12*, 59
* philosophy and medicine, *1*, 117
*Biometric studies in psychiatry. VII. Tuberculosis, syphilis, and cancer, *4*, 208; VIII. The weight of the endocrine glands, *6*, 489
* on army officers—Somatological norms, correlations, and changes with age, *4*, 509; Somatological norms in disease, *5*, 61
* study of the endocrine organs in relation to mental disease, *7*, 350, 555
* of the relation between oral and rectal temperatures in normal and schizophrenic subjects, *7*, 24
* of the relation between the excretion of creatinine nitrogen and several body measurements, *4*, 351
*Biophysical factors in relation to functional imbalances, *10*, 93
*Biostatistics of senility, *11*, 1
Birth control, *see* Contraception
Birth data for Melanesians, *3*, 357
-death ratio, New World populations, *12*, 375
injury in Negroes, *4*, 26
number of, and age of mother, Puerto Rico, *20*, 24
order, age of mother, and socio-economic status, *13*, 64
and developmental anomalies, *1*, 382
* and heart rate in children, *15*, 171
and stillbirth rates, by race, *17*, 73, 95
* of Mongol and other feebleminded children, *8*, 256
rates and contraception, *6*, 387
* and density in a logistic population, *4*, 119
and family income, *4*, 541
and occupation of father, in Japan, *2*, 511
in Maryland, *12*, 193
and thyroid disorder, *5*, 516
Chinese, *19*, 3

correlation with growth-rate of population, *3*, 184
differential, *12*, 190
generation, *19*, 133
in a logistic population, *3*, 461, 481
in New York City, *5*, 94
Korean, *3*, 325
Parsi, *20*, 56
Puerto Rico, *17*, 278
records, San Juan Pueblo, *12*, 142
registration, completeness of, *12*, 40
statistics of primitive peoples, *5*, 58
weight and neonatal growth, *11*, 24
Births, illegitimate, estimation of, *12*, 47
premature, and age of father, *11*, 350
BLACK, DAVIDSON, Interim Report on the Skull of Sinanthropus, *2*, 439-440
Blood characters and body measurements, *2*, 277
counts, hourly variation in, *10*, 514
* inter- and intra-individual variability of, *8*, 581
factor, Rh, inheritance of, *17*, 195
group(s), African population, *12*, 470
* and types in Baghdad and vicinity, *13*, 398
B, introduction into Europe, *14*, 413
comparison of, among Jewish and Gentile students, *16*, 173
* frequencies of European peoples, and racial origins, *12*, 457
genes, nomogram for, *6*, 558
indices and racial classification, *2*, 130
inheritance of, *5*, 577; *15*, 222; *17*, 13
of Negroes, *4*, 10
* of the Maori, *3*, 282
* of Tungus, *8*, 273
sampling error of frequencies of, *12*, 459
levels, sulfonamides, *18*, 171
pressure, and chemical poisoning, *19*, 27
control charts in industrial medicine, *19*, 27
in healthy males, *5*, 541
* in old age, *2*, 264
of army officers, *4*, 518
of cardiacs and non-cardiacs, *8*, 44
of Japanese women, *14*, 324
systolic, during pregnancy, *8*, 232
BLUMENTHAL, FRITZ, Racial Differences in Resistance to Respiratory Infection, *14*, 104-109
BOAS, FRANZ, Studies in Growth, *4*, 307-350
Studies in Growth II, *5*, 429-444
Studies in Growth III, *7*, 303-318
The Cephalic Index in Holland and Its Heredity, *5*, 587-599
Bodily and gastric activity of new-born infants, *8*, 368-380, 381-386

(Bodily)
dimensions, male and female Negroes, *15*, 128
*Body build and diagnosis of schizophrenia, *11*, 357
and weight, *11*, 473
* in eczema (Body build in infants, IX), *11*, 269
* in infants, *6*, 612; *11*, 269
in schizophrenia, *11*, 357; *14*, 192
of Negroes, *4*, 5
of women students, *5*, 122
development, geographic differences in, *9*, 43
habitus types, and infant mortality, *12*, 243
length, *see also* Stature, Height
and heart disease, *8*, 38
and menarcheal age, *8*, 199
daily decreases in, *15*, 65
erect and supine, *4*, 262
of Japanese women, *14*, 289
of mental patients, *7*, 362; *14*, 29
of twelve-day old babies, *8*, 436
measurements and nitrogen excretion, *4*, 351
parts, diameters of, *10*, 360
size, of langurs and macaques, *14*, 446
specific gravity of human, *5*, 646
volume, of different disease groups, *6*, 683
weight, *see also* Weight
and heart disease, *8*, 38
and menarcheal age, *8*, 199
correlation with blood characters, *2*, 277
of different disease groups, *6*, 663
of girls, normal growth in, *12*, 280
of infants, *11*, 24
Bolles, Marjorie, *see* LANDIS, CARNEY
Bone fractures in great apes, *9*, 286
volume, of finger tips, *10*, 295
* weight in the human carpus, *7*, 108
VON BONIN, GERHARDT, European Races of the Upper Paleolithic, *7*, 196-221
von Bortkewitsch, L., (law of small numbers), *19*, 154
Bound, Louise Emerson, *see* KELLY, HARRIET J.
BOYD, EDITH, The Specific Gravity of the Human Body, *5*, 646-672
Boyd, Lyle G., *see* BOYD, WILLIAM C.
BOYD, WILLIAM C., Nomogram for Rapid Computation of the Blood Grouping Genes in Populations, *6*, 558-561
and BOYD, LYLE G., Blood Groups and Types in Baghdad and Vicinity, *13*, 398-404
Boynton, Bernice, *see* MEREDITH, HOWARD V.
BOYNTON, PAUL L., and HERBERT, KITTY GALE, Correlational Analyses of the Influence of Basal Chronological Age on IQ Relationships to Specified Anthropometric Measurements, *14*, 527-531

Brachial index, in langurs and macaques, *14*, 455
in primates, *9*, 298
Brachio-antebrachial musculature, phylogeny of, *13*, 37
Brachitanytes, subgenus of gibbons, *5*, 216, 417
Brachyphalangy, contrasted with apical dystrophy, *4*, 202
Brain disease, organic, causing reversion to primitive behavior, *1*, 406
* potentials (Some physiological phenomena fitted to growth equations, II) *10*, 145
size and shape, and jaw protrusion, *2*, 229, 237
weight, and age, *14*, 59
human, for selected causes of death, *14*, 238
variation in, with hours post mortem, *14*, 61
BRANDT, WALTER, The Prenatal Development of the Hair Tracts in Primates, *12*, 203-232
Brazil, population, *12*, 366
Breadths, biacromial and biiliac, of different disease groups, *6*, 691
Brewery employees, mortality of, *1*, 136
British and Foreign Medical Review, *6*, 637
Brothers in *Who's Who*, *5*, 371
Brown, Addison W., *see* MEREDITH, HOWARD V.
Browne, Sir Thomas, on physiognomy, *1*, 283
BRUNNER, ENDRE K., The Outcome of 1556 Conceptions: A Medical and Sociological Study, *13*, 159-176
BURCH, GEORGE E., COHN A. E., and NEUMANN, C., A Study of the Total Volume of the Human Finger Tip and Toe Tip, *13*, 526-532
and SODEMAN, WILLIAM A., The Correlation of Bone Volume and Soft Tissue Volume in the Human Finger Tip, *10*, 295-298
Burch, George E., *see also* WINSOR, TRAVIS
Bureau of Human Heredity, *11*, 287
BURGESS, ERNEST W., and WALLIN, PAUL, Personal Appearance and Neuroticism as Related to Age at Marriage, *16*, 15-22
BURKS, BARBARA S., Social Promotion in Relation to Differential Fecundity, *13*, 103-113
and JONES, HAROLD E., A Study of Differential Fertility in Two California Cities, *7*, 539-554
BURROW, TRIGANT, Bio-physical Factors in Relation to Functional Imbalances, *10*, 93-105
Neuropathology and the Internal Environment, *7*, 74-94

CALABRESI, MASSIMO, The Relation of Country of Origin to Mortality for Various Causes in New York State, *17*, 340-367
*Calcification of the first costal cartilage among whites and Negroes, *6*, 543
Calcium and phosphorus balances in pregnancy, *9*, 29
*Calculus of variations and the logistic curve, *11*, 173
California cities, differential fertility in, *7*, 539
Indians, censuses of, *15*, 159
migration and urbanization, *15*, 33

(California Indians)
racial fusion among, *15*, 153
Cameroons, gorillas from, *15*, 322
Campaigns and battle casualties, pre-colonial Mexico, *18*, 101
Canada, population, *12*, 376
*Cancer and marital status, *15*, 73
and weight, *2*, 173
in Army officers, *5*, 89
in Negroes, *1*, 231; *3*, 216
in the insane, *4*, 217, 221
mortality by country of origin, *17*, 351
occurrence among radium miners, *11*, 134
of digestive tract, and sex, *2*, 85
CANDELA, P. B., The Introduction of Blood-Group B into Europe, *14*, 413-443
Canine diastemata, *12*, 328
Canning, Richard G., *see* CHENOWETH, LAURENCE B.
Cap Blanc skeleton, *7*, 197
Cardiac cycle, duration of, in relation to birth order, *15*, 172
*Cardiovascular disease mortality in a sample of white males and their parents, *13*, 350
-renal diseases in Army officers, *5*, 69
Caries frequency, in ancient and modern Greeks, *16*, 286
in Porto Rican children, *5*, 274
Carious lesions in ancient man, *12*, 314
Carmichael, Hugh T., *see* LINDER, FORREST E.
CARPENTER, C. R., An Observational Study of Two Captive Mountain Gorillas (*Gorilla beringei*), *9*, 175-197
Carpus, bone weight in, *7*, 108
CATES, H. A., and GOODWIN, J. C., The Twelve-Day Old Baby, *8*, 433-450
CATTELL, PSYCHE, Preliminary Report on the Measurement of Ossification of the Hand and Wrist, *6*, 454-471
Causes of death, organological classification, *1*, 237
Cebus, relative growth, *19*, 53
Cedarstrand, Grace, *see* KIRKPATRICK, CLIFFORD
Census, California Indians, *15*, 34
United States, 1830, *14*, 353
Center of gravity of body, *10*, 361; *11*, 530
Central American and Caribbean area, population, *12*, 365
* Malaria Library, *6*, 240
Cephalic index, growth changes in, *8*, 1; *11*, 201
* in Holland and its heredity, *5*, 587
measurements, of "Old American" morons, *13*, 464
of West African Negroes, *9*, 484
Cerebral cortex and behaviorism, *2*, 461
hemorrhage and weight, *2*, 171
in Negroes, *3*, 227
*Changes in systolic blood pressure, heart rate and temperature before, during and after pregnancy in a healthy woman, *8*, 232

* in the incidence of mortality at advanced ages, *1*, 558
* in the sex ratio in infant mortality according to age, *3*, 560
CHASSAN, JACOB B., The Autocorrelation Approach to the Analysis of the Incidence of Communicable Diseases, *20*, 90-108
CHEESEMAN, E. A., The Effect of the War Years upon Mortality in Childhood, *10*, 538-546
Chemehuevi Indians, mortality among, *3*, 397
Chemical exposure, statistical techniques in detecting, *19*, 27
*Chemoprophylaxis in the prevention of disease with especial reference to meningococcal infection. I. A comparative study of the absorption, persistence and excretion of four sulfonamide compounds, *18*, 171
CHENOWETH, LAURENCE B., and CANNING, RICHARD G., Relation of Season of Birth to Certain Attributes of Students, *13*, 533-540
Chenoweth, L. B., *see also* MILLS, C. A.
Chest circumference, and body build, *11*, 473
growth in, *1*, 462
of Cape colored people, *11*, 329
of different disease groups, *6*, 681; *8*, 60
of schizophrenics, *11*, 365; *14*, 30
depth of different disease groups, *6*, 692; *8*, 69
of morons, *13*, 464
measurements, Army officers, *4*, 515
growth in, *13*, 306
Japanese women, *14*, 314
schizophrenic women, *14*, 30
method for computing cross-section of, *2*, 539
width, pubescent and non-pubescent girls, *9*, 27
Child mortality, San Juan pueblo, *12*, 171
effect of war years on, *10*, 538
Children, desire for, as factor in selection, *1*, 512
Porto Rican, growth and nutrition of, *4*, 469
dental caries in, *5*, 274
proportion in aboriginal and American Indian populations, *19*, 84
stature of, Ottawa, *19*, 74
Toronto, *16*, 126
Chimpanzee, dermatoglyphics, *10*, 457
hair tracts of, *12*, 207
measurements of, *11*, 380
sex behavior of, *11*, 78
*Chinese constitutional differentiation and Kretschmerian typology, *9*, 451
family life tables, *3*, 157
genealogy, *4*, 41
population, *19*, 1
Chiromys madagascariensis, hair tracts of, *12*, 205
*Chronic sickness in relation to survivorship twenty years later, *18*, 33
*Chronology of physiologic events (Studies in the physical development, VI), *17*, 147

*Church(es) and social well-being, *4*, 421
* membership and commitments of prisoners, *3*, 429
* and the homicide rate, *1*, 562
*Cicatrix of the neck associated with anomalous dental occlusion in one of a pair of monozygous twins, *20*, 36
Ciocco, Antonio, A Statistical Approach to the Problem of Tone Localization in the Human Cochlea, *6*, 714-721
Birth-Order and Heart Rate in Children, *15*, 171-174
Cardiovascular Diseases Mortality in a Sample of White Males and Their Parents, *13*, 350-362
Chronic Sickness in Relation to Survivorship Twenty Years Later, *18*, 33-48
On Human Social Biology. II. Disruptive and Cohesive Factors in the Marital Group, *10*, 555-574; III. Elements Affecting the Formation of the Marital Group, *11*, 234-248
On the Distribution of the Differences between Birth Date and Death Date: Note on an Inquiry by the Late Raymond Pearl, *13*, 247-251
On the Interdependence of the Length of Life of Husband and Wife, *13*, 505-525
On the Mortality in Brother-Sister and Husband-Wife Pairings, *13*, 189-202
On the Mortality in Husbands and Wives, *12*, 508-531
Parity Order of Birth, Age of Mother, and Socio-Economic Status, *13*, 64-87
Studies on Constitution. III. Somatological Differences Associated with Diseases of the Heart in White Females, *8*, 38-91
Studies on the Biological Factors in Public Health. I. The Trend of the Age of Marriage in Washington County, Maryland, from 1897 to 1938, *12*, 59-76
The Masculinity of Stillbirths and Abortions in Relation to the Duration of Uterogestation and to the Stated Causes of Foetal Mortality, *10*, 235-250
The Trend in the Proportional Contribution of the Socio-Economic Groups to Natality. A Report Based on the Births in Washington County, Maryland, from 1896 to 1938, *12*, 188-203
Variation in the Sex Ratio at Birth in the United States, *10*, 36-64
Hughson, W., and Palmer, C., Studies on Pupils of the Pennsylvania School for the Deaf. 2. Resemblances in the Auditory Acuity of Siblings, *11*, 259-268
Ciocco, Antonio, *see also* Pearl, Raymond
Circulatory system in old age, *4*, 88, 101
Circumference of upper arm, Eurafrican juveniles, *11*, 331
Cirrhosis of liver and weight, *2*, 175
Clark, Grace, Method of Computing the Cross-Section of the Chest, *2*, 539-546
*Classification and code of occupations, *5*, 491
by disease groups, *1*, 126
of gibbons and siamangs, *5*, 212, 385
of races, *8*, 348
Clavicle, in langurs and macaques, *14*, 450
of primates, *2*, 336
Claviculo-humeral index in primates, *9*, 293
Cleidocranial dysostosis, and wormian bones, *18*, 61
inheritance of, *18*, 103

CLEMENTS, FORREST, Racial Difference in Mortality and Morbidity, *3*, 397-419
Climate, and birthplace of notables, *1*, 545
Climatic changes in Europe and Asia, *2*, 55
Clinical records in constitution study, *1*, 26
*Cobbes Questions and Answers, *2*, 137
Cochlea, tone localization in, *6*, 714
Coefficient of group divergence, *7*, 99
of inbreeding, and intelligence, *20*, 175
of racial likeness, *7*, 95
*Coefficients of correlation between nitrogenous constituents of the urine after ingestion of low, normal, and high protein diets, *7*, 419
COHEN, B. M., FAIRBANK, RUTH E., and GREENE, ELIZABETH, Statistical Contributions from the Mental Hygiene Study of the Eastern Health District of Baltimore. III. Personality Disorder in the Eastern Health District in 1933, *11*, 112-129; IV. Further Studies on Personality Disorder in the Eastern Health District in 1933, *11*, 485-512.
Cohn, A. E., *see* BURCH, G. E.
Coitus frequency per month, *12*, 549
and age of wife, *12*, 547
College grades and beauty, *10*, 68
men, height and weight of, *5*, 445
students, families of, *3*, 342
women, height and weight of, *5*, 122, 600
COLLINS, SELWYN D. and PENNELL, ELLIOTT H., The Use of the Logistic Curve to Represent the Prevalence of Defective Vision among Persons of Specific Ages above 30 Years, *7*, 257-266
Color blindness, inheritance of, *5*, 130
in Negroes, *8*, 451
fusion in synesthesia, *5*, 183
hearing, *5*, 155
Columbus, Ohio, fertility study, *6*, 595
*Comments on the unhappiness of Mr. Fraser Roberts, *14*, 113
Common cold in Jews and non-Jews, *4*, 429
*Common misconception concerning the behavior of linked factors, *11*, 546
Communicable disease incidence, autocorrelational analysis, *20*, 90
*Comparative biometrical study of physique of Japanese women born and reared under different environmental conditions, *14*, 279
* examination of certain aspects of the populations of the new World, *12*, 359
method in biology, importance of, *2*, 458
* physical growth in Eurafrican and European juvenile adults, *11*, 319
* study of finger prints and toe prints, *8*, 531
* of the absorption, persistence and excretion of four sulfonamide compounds, *18*, 171
* of the mortality of the population of Greece, *8*, 553
* of the physical status of East Tennessee mountain children, *5*, 706
*Comparison of groups in respect of a number of measured characters, *7*, 95
* of the frequencies of certain genetic traits among Gentile and Jewish students, *16*, 172

Compensation theory of synesthesia, *5*, 161
Completed generation reproduction rates, *19*, 133
Compound Poisson, deaths calculated by, *11*, 21
*Compulsions, obsessions and feelings of unreality, *1*, 514
Computation of probable errors of differences, *11*, 543
Computed average longevity (CML), *6*, 162
*Concept of organism-as-a-whole, its application to clinical situations, *8*, 489
*Conceptions, outcome of 1556, *13*, 159
*Concerning a recently reported finding on the physical growth of boys, *9*, 567
Concordance-discordance ratios, in diagnosis of zygosity, *15*, 234
*Congenital anomalies, contribution to study of etiology of, *1*, 278
debility in Negroes, *3*, 233
hemiatrophy, *11*, 459
malformations in Negroes, *4*, 26
Congress for Unity of Science, *11*, 146
Conjoined twins, *situs inversus viscerum* in, *12*, 29
Consanguinity, tables of, in genetic studies, *2*, 259
Consciousness, disturbance of, in petit-mal attacks, *2*, 547
Constancy of age at onset of disease within a family, *4*, 76
Constitution and culture, *4*, 243
and diseases of the heart, *6*, 650; *8*, 38
study, methods, *1*, 10
*Constitutional factors in mortality at advanced ages (Studies in human longevity, V), *4*, 80
somatology, *1*, 283
study of psychosis, record sheet, *12*, 204
types, *1*, 130; *9*, 456
*Contour maps, center of gravity, moment of inertia and surface area of the human body, *10*, 356
Contraception, and fertility, *10*, 372
* in 2000 women, *4*, 363
* in 4945 married women, *6*, 355
in a rural sample, *14*, 1
in Puerto Rico, *20*, 21
*Contribution to the analysis of the exceptions to Bernstein's theory, *17*, 13
Control charts in preventive medicine, *19*, 27
* of conception in a selected rural sample, *14*, 1
Convulsions in Negro infants, *4*, 26
Coogan, Marguerite, *see* WILLOUGHBY, RAYMOND R.
COOK, S. F., Human Sacrifice and Warfare as Factors in the Demography of Pre-colonial Mexico, *18*, 81-102
Migration and Urbanization of the Indians in California, *15*, 33-45
Racial Fusion among the California and Nevada Indians, *15*, 153-165
Survivorship in Aboriginal Populations, *19*, 83-89
The Population of Mexico in 1793, *14*, 499-515
COOLIDGE, HAROLD J., JR., Notes on a Family of Breeding Gibbons, *5*, 288-294

Copulation in chimpanzee, *11*, 95
of gibbons, *5*, 290
*Correlation and influence of time since feeding (Relationship between bodily and gastric activity of newborn infants, I), *8*, 368
arithmetic and logarithmic, *14*, 96
* between growth rates of population and vital factors, by geographical districts, *3*, 180
* between intelligence and fertility, *12*, 114
* growth changes in physical, *1*, 462
* in growth, *19*, 260
* of characters of the blood with body-weight, stature, and surface-area, *2*, 277
of death rates, *2*, 505, 525
* of sums, *2*, 134
*Correlational analyses of the influence of basal chronological age on IQ relationships to specified anthropometric measurements, *14*, 527
Costal cartilage, calcification of, *6*, 543
COUNT, EARL W., A Quantitative Analysis of Growth in Certain Human Skull Dimensions, *14*, 143-165
Growth Patterns of the Human Physique: An Approach to Kinetic Anthropometry, Part I, *15*, 1-32
Country of origin and mortality, *17*, 340
Courtship of Mohave homosexuals, *9*, 513
COX, EUGENE A., Political Parsons, *1*, 284-285
Cradle of man, *1*, 5
*Cranial capacities by both plastic and water techniques with cranial linear measurements of the Reserve collection: white and Negro, *14*, 423
capacity of gorilla, *15*, 240
* index; a statistical study, *13*, 88
of gorilla, *15*, 307
of Negroes and whites, *14*, 482
measurements and indices of ancient skulls, *12*, 317
of crania with wormian bones, *18*, 66
measures, ancient Greek, *18*, 6
relationships and correlation, *6*, 308
variability, of gorilla, *15*, 313
Craniology, Mongolian, *1*, 421
*Mr. Cranium's lecture, *1*, 139
Creatine and creatinine in pregnancy, *9*, 404; *10*, 530
and protein intake, *7*, 419
Creatinine, excretion of, and body measurements, *4*, 351
CRESSMAN, L. S., Some Effects of Thyroid Disorder in Women, *3*, 529-546
and SPENKER, E. C., On Some Quantitative Evidence of the Effect of Thyroid Disorder upon the Birth Rate, *5*, 516-519
Crime and church membership, *3*, 429
*Critique of Professor Wood-Jones' paper, *2*, 99
Crockett, Hazel E., *see* HARVEY, O. L.
CROMWELL, HOWARD, and RIFE, DAVID C., Dermatoglyphics in Relation to Functional Handedness, *14*, 516-526

CROOKS, KENNETH B. M., Further Observations on Color Blindness among Negroes, with Genealogic and Geographic Notes, *8*, 451-458
*Cross, John, M. D., on physiognomy and constitution, *1*, 426
Crural index, in langurs and macaques, *14*, 457
in primates, *9*, 300
*Cultural aspects of birth control in Puerto Rico, *20*, 21
CUMMINS, HAROLD, Dermatoglyphics in North American Indians and Spanish-Americans, *13*, 177-188
Morphology of the Palmar Hypothenar Dermatoglyphics in Man, *7*, 1-23
and SPRAGG, S. D. S., Dermatoglyphics in the Chimpanzee: Description and Comparison with Man, *10*, 457-510
Cummins, Harold, *see also* SHANKLIN, WILLIAM M.
see also RIFE, DAVID C.
*Current versus generation life tables, *13*, 435
Curtailment of life by different causes of death, *6*, 66
Curtate distributions, constants of, *9*, 410
Curve, human growth, *15*, 2
of national growth, *18*, 204
Cycle, seasonal, in human growth, *2*, 523
Cyclothymes, Chinese, *9*, 459

DAHLBERG, GUNNAR, An Analysis of the Conception of Race and a New Method of Distinguishing Races, *14*, 372-385
Dahomey, population statistics of, *4*, 252
*Daily decrease in the body total and stem lengths of normal children, *15*, 65
DALTON, MARY, Mortality in New York City a Century and a Quarter Ago, *6*, 87-97
DAMON, ALBERT, A Note on the Estimation of Dysplasia in Human Physiques: Sheldon's Method and the Analysis of Variance, *14*, 110-112
Physique in Hereditary Mental Defect: An Anthropometric Study of 97 "Old American" Female Morons, *13*, 459-472
Dark adaptation, equation for, *11*, 403
"Das Gesetz der kleinen Zahlen," *19*, 154
DAVENPORT, CHARLES B., Do Races Differ in Mental Capacity? *1*, 70-89
The Development of Trunk Width and the Trunk Width Index, *7*, 151-195
The Thoracic Index, *6*, 1-23
and MINOGUE, BLANCHE M., Intelligence Quotient and Physical Quotient, *2*, 473-507
DAVIS, H. JACKSON, Blood Pressure in Old Age, *2*, 264-276
*Death, an adaptation, *11*, 408
age distribution of, in aboriginal and American Indian populations, *19*, 85
in American Indians, *7*, 335; *8*, 223
causes of, among Indians, *3*, 411
New York, 1804-08, *6*, 87
of husbands and wives, *12*, 519; *15*, 521
data for Melanesians, *3*, 361

of parents and offspring, from heart disease, *13*, 357
probabilities in closed population groups, *18*, 158
rate(s), among old-age assistance recipients in Rhode Island, *15*, 175
and mortality sex ratio, *1*, 97
correlation with growth-rate of population, *3*, 185
from cancer, by sex and marital status, *15*, 74
in China, *19*, 3
in Korea, *3*, 327
in logistic population, *3*, 470, 483
in New York City, *5*, 94
in Puerto Rico, 17, 278
in San Juan pueblo, *12*, 165
of children, effect of war years on, *10*, 543
of early man, *5*, 44
of Parsi, *20*, 56
secular changes in, *3*, 494
use of life table, for comparative mortality, *18*, 127, 238
records for New England towns, 1826-1835, sources of, *14*, 367
*Decline in average length of life, *2*, 199
in infant mortality, and public health procedure, *9*, 154
in natality and mortality, Washington County, Md., *12*, 63
Decrement, of alveolar arches, *10*, 340
Degeneracy theory of synesthesia, *5*, 160
Degenerative heart diseases in Army officers, *5*, 74
Dementia praecox, in Negroes, *7*, 503
Demography of the Parsi, *20*, 49
*Density of hair in primates, *3*, 303
of hunting populations, *5*, 38
of population, New World, *12*, 364
correlation with growth-rate of population, *3*, 193
Dental and skeletal age, of gorilla, *16*, 42
* caries in Puerto Rican children, a study of, *5*, 274
development in gorilla, *15*, 236, 307
occlusion associated with a cicatrix of neck, *20*, 36
pathology of ancient and modern Greeks, *16*, 285
phylogenetic relationships, *12*, 324
Dentition and facial growth, *20*, 109
inherited reductions in, *6*, 627
* of identical twins, with particular reference to an identical pathological condition, *5*, 629
patterns of primates, *2*, 106
DePorte, J. V., Marriages in the State of New York with Special Reference to Nativity, *3*, 376-396
and Parkhurst, Elizabeth, Homicide in New York State, *7*, 47-73
*Depressions, weather and health, *10*, 388
Dermatoglyphics, *see also* Finger prints
* and mirror-imaging, *15*, 55

(Dermatoglyphics)
in Mongoloid imbeciles, *20*, 146
* in North American Indians and Spanish-Americans, *13*, 177
* in relation to functional handedness, *14*, 516
* in Rwala Bedouins, *9*, 357
* in the chimpanzee: description and comparison with man, *10*, 457
in twin diagnosis, *10*, 12
morphology of, *7*, 1
of chimpanzee, *10*, 457
of triplets, *5*, 10
percentages of patterns, among Gentile and Jewish students, *16*, 176
Descendants, tables of, in genetic studies, *2*, 250
D'Esopo, D. A., *see* LANDIS, CARNEY
Deterioration of synesthesia with age, *5*, 194
*Development of the alveolar arches in normal and abnormal occlusion, *10*, 327
* of the head in the same individuals, *11*, 197
* of trunk width and the trunk width index, *7*, 151
*Developmental anomalies and primogeniture, *1*, 382
differences between Negroes and whites, *4*, 9
DEVEREUX, GEORGE, Institutionalized Homosexuality of the Mohave Indians, *9*, 498-527
Dextrality, *see* Handedness
Diabetes, and weight, *2*, 174
in Negroes, *3*, 224
mortality, by country of origin, *17*, 350
Diagnosis of identical twins, *5*, 29; *15*, 221
Diarrhea in Negro infants, *4*, 27
Dichotomy, multiple, factorial analysis of, *20*, 195
Diederich, Gertrude Wylie, *see* STRANDSKOV, HERLUF H.
DIEHL, HAROLD S., Heights and Weights of American College Men, *5*, 445-479
Heights and Weights of American College Women, *5*, 600-628
Diet, adequacy of children's, *15*, 298
and fecundity, of early man, *5*, 52
DIETHELM, OSKAR, Disturbance of Vision and Consciousness in Petit-mal Attacks, *2*, 547-554
*Differences in the utilization of calcium and phosphorus in negative and positive balances during pregnancy, *9*, 29
Differential birth rate, Washington County, Maryland, *12*, 188
fecundity, and social promotion, *13*, 103
fertility in Japan, *2*, 508
* in two California cities, a study of, *7*, 539
intelligence and, *12*, 114
method, Weinberg's, *12*, 248
* mortality in the American Negro, *3*, 71, 203
survival rate, *1*, 508
Difficulties of ape-man theory, *1*, 4, 155
*Digby, Kenelm, seventeenth century psychotherapist, *5*, 506
Digestive diseases of Negroes, *3*, 224

tract, diseases of, and sex, *2*, 67
Digital webbing of gibbons and siamangs, *5*, 252
Dill, D. B., *see* ROBINSON, S.
Dimorphism, in gorilla skulls, *15*, 323
Diphtheria, in Negroes, *3*, 85; *4*, 24
Discharge rates of mental patients, in Georgia, *11*, 519
Discordance as a measure of penetrance, *19*, 217
Disease, as maladjustment of organism and environment, *1*, 122
as reactive process, *1*, 450
inheritance and severity of, *4*, 69
mental, somatology of schizophrenics, *14*, 23
* of upper respiratory tract (common cold) in Jews and non-Jews, *4*, 429
personification of, *1*, 123
records of twins, *3*, 270
somatological norms in, *5*, 61
*Distribution and correlation of variation in the total immediate ancestral longevity of nonagenarians and centenarians, in relation to the inheritance factor in duration of life (Studies on human longevity, VI), *6*, 98
* of deaths among American Indians, *8*, 223
* of sex ratios within families in an Ohio city (Studies in human inheritance, XVI), *9*, 99
of slope constants, *12*, 264
of stillbirths and stillbirth rates by age of parents *11*, 346
* of the differences between birth date and death date: note on an inquiry by the late Raymond Pearl, *13*, 247
*Disturbance of vision and consciousness in petit-mal attacks, *2*, 547
DITTRICH, R. J., Congenital anomalies, *1*, 278-282
*Do races differ in mental capacity? *1*, 70
Doctor and patient, relation between, *1*, 459
Dominance of N. pigmentation, *1*, 361
-subordination, *11*, 105; *19*, 196
DORN, HAROLD, Cancer and Marital Status, *15*, 73-79
Double monster, mirror imaging in, *12*, 31
quantitative study of a, *2*, 19
DOVE, W. FRANKLIN, Appetite Levels of Food Consumption: A Technique for Measuring Foods in Terms of Psychological and Nutritional Values Combined, *15*, 199-220
DOW, PHILIP, and TORPIN, RICHARD, Placentation Studies: Correlation between Size of Sac, Area of Placenta, Weight of Placenta and Weight of Baby, *11*, 248-258
DRAPER, GEORGE, Biological Philosophy and Medicine, *1*, 117-135
Drepanocytanemic habitus syndrome, *16*, 112
Drosophila life tables, *11*, 5
sex ratio of offspring and longevity of parents, *12*, 418
Dryopithecus, *1*, 155, 158
DUBLIN, LOUIS I., Influence of Weight on Causes of Death, *2*, 159-184
and LOTKA, ALFRED J., The History of Longevity in the United States, *6*, 43-86

(DUBLIN, LOUIS I.)
and SPIEGELMAN, MORTIMER, Current versus Generation Life Tables, *13*, 439-458
DUPERTUIS, C. W., ATKINSON, WILLIAM B., and ELFTMAN, HERBERT, Sex Differences in Pubic Hair Distribution, *17*, 137-142
Dura, volume of, *14*, 482
Duration of life, see Life table, Longevity
of reflex, equation for, *9*, 551
Dutch-white, body build of, comparison with Negroes, *15*, 127
DWORKIN, SAMUEL S., Folk Medicine in Russia, *3*, 576
Dynamics of arm movements, *11*, 534
Dysostosis, cleidocranial, *18*, 61, 103
Dysplasia, Sheldon's method for estimating, *13*. 405
* in human physiques, estimation of, *14*, 110

Ear dimensions of different disease groups, *6*, 694
length and breadth, *8*, 71
Earth temperature and human growth, *13*, 374
Eastern Health District of Baltimore, migration rates for families, *15*. 101
permanence of residence, *19*, 91
personality disorder in, *11*, 112, 485
*Ecology of American notables, *1*, 544
Economic status and fertility, *4*, 525
and personality disorder, *11*, 123, 499
and weight gain, *12*, 532
of Negroes, *4*, 4
Eczema, occurrence in sexes, *11*, 269
Education of family head, and permanence of residence, *19*, 110
*Effect of contraception upon human fertility, *10*, 372
* of the war years upon mortality in childhood, *10*, 538
Egyptian crania, *6*, 309
*Eighteenth-century French evolutionist, An, *2*, 559
*Elementary problems of importance to small sample practice, *20*, 205
ELFTMAN, HERBERT, The Function of the Arms in Walking, *11*, 529-535
Elftman, Herbert, *see also* DUPERTUIS, C. W.
ELSBERG, C. A., and SPOTNITZ, H., The Growth Formula of Bertalanffy and Its Similarity to Equations for Excitation and Recovery in the Central Nervous System, *11*, 402-407
Embryonic growth, of human foot, *16*, 1
Emotional tone theory of synesthesia, *5*, 165
Emotionality, spousal estimation of *10*, 417
*Endocrine glands, weight of, *6*, 489
* organ weights, and somatological habitus types (Studies on constitution, IV), *8*, 92
organs and mental disease, *7*, 350, 555
patterns of insane, *7*, 594
system in old age, *4*, 94, 107
Endogamy, and formation of types, *1*, 254

ENGLE, EARL T., A Menstrual Record during the Menopause, *9*, 564-566
and SHELESNYAK, M. C., First Menstruation and Subsequent Menstrual Cycles of Pubertal Girls, *6*, 431-453
English life table, *11*, 10
Enklewitz, Morris, *see* LASKER, MARGARET
*Entrenched Negro physical features, *1*, 57
Environment and response, *1*, 118
contact of organ systems with, *4*, 111
factors of, influencing mortality sex ratio, *1*, 100
internal and neuropathology, *7*, 74
of twins, heredity as a factor in, *6*, 324
*Environmental trends among the American Negro (Studies in the physical development, VII), *17*, 207
Epidemics, in San Juan pueblo, *12*, 167
Epilepsy and maternal age, *18*, 49
disturbances of vision and consciousness in, *2*, 547
folklore of, *2*, 111
Epiphysis, weight of, *6*, 492; *7*, 581
Equations for excitation and recovery in the central nervous system, *11*, 402
Erect posture as a result of arboreal life, *2*, 232
Eruption of permanent dentition, *2*, 485
Erysipelas in Negroes, *3*, 92; *4*, 28
Erythrocyte counts in man, variability of, *8*, 581
Eskimos, anthropometry of, *5*, 313
*Essential fructosuria, *13*, 51
*Estimates of intra-individual and inter-individual variation of the erythrocyte and leukocyte counts in man, *8*, 581
Ethnic groups, amalgamation of, *7*, 437
ancient Greek, *18*, 1
classification of, *13*, 390
Ethnogenics, *4*, 244
Etymology in cultural history, *2*, 110
Eugenic(s) character of selection, *1*, 506
circa 590 B. C., *1*, 142
research survey, *3*, 288
Eurafrica, migration into, *2*, 49
Eurafrican juveniles, growth of, *11*, 319
Europe, races of, *2*, 53
*European(s) races of the Upper Paleolithic, *7*, 196
weight of parathyroid glands of, *9*, 246
*Evaluation of slopes and intercepts of straight lines, *19*, 231
* of the Cancer Educational Program in Massachusetts, *16*, 115
*Evolution as a maximal principle, law of, *17*, 167
functional theories of, *2*, 223
Evolutionary specialization of gibbons and siamangs, *5*, 212, 385
Exercise, adaptations to, *13*, 139
*Expectation of life and probability of death, *18*, 238

(Expectation of life)
at birth, in generation life table, *13*, 449
Exposure to risk of pregnancy, *4*, 401, 532; *6*, 366
Extensors of forearm, phylogeny of, *13*, 23, 203
*External genitalia of the simian primates, *8*, 309
*Extraordinary case of obesity and a review of some lesser cases, *14*, 166
Extremities, transverse growth of, *9*, 366
upper, of children, *11*, 277
Eyeball, human, weight and growth of, *12*, 1

Face width, changes among immigrants, *7*, 313
*Facial growth in children of five to eight years of age, *20*, 109
measurements and indices of ancient skulls, *12*, 317
size of langurs and macaques, *14*, 460
*Factorial analysis of a multiple dichotomy, *20*, 195
* of anthropological measurements in psychotic patients, *18*, 133
of physiological responses, *14*, 76
of vital indices and social factors, *5*, 115
Factors, genetic, for twinning, *11*, 179
Fairbank, Ruth E., *see* COHEN, B. M.
Falling sickness, names of, *2*, 109
Familial heart diseases, *13*, 350
*Families of freshmen, *3*, 342
Family size and education of parents, *12*, 117
and permanence of residence, *19*, 112
and personality disorder, *11*, 493
in logistic population, *3*, 478
of San Juan Pueblo Indians, *12*, 156
* studies in the Eastern Health District. III. A consideration of issues involved in determining migration rates for families, *15*, 101; IV. Permanence of residence with respect to various family characteristics, *19*, 91
Fatigue of mother, and fetal activity, *18*, 221
Fatness, relative degree of, *14*, 167
*Fecundity and fertility of early man, *5*, 35
differential, and social promotion, *13*, 103
FEDERICI, NORA, Parental Age and Prenatal Mortality, *12*, 430
Feebleminded children, birth order of, *8*, 257
Feeding of Negro infants, *4*, 4
*Female-male index of body build in Negroes and whites: an interpretation of anatomical sex differences, *15*, 127
Females, handedness and dermatoglyphics, *14*, 518
Femur of primates, *2*, 367
Fertility and contraception, *4*, 363; *6*, 354; *10*, 373
and economic status, *4*, 525; *12*, 199
and homogamy, *8*, 19
and longevity, *12*, 412
* in married women dying after the end of the reproductive period, *7*, 392
contour surfaces, *8*, 523

differential, in California, *7*, 539
in Sendai, Japan, *2*, 508
* in relation to age at time of marriage, *8*, 420
in San Juan Pueblo, *12*, 163
intelligence and, *12*, 114
measured by completed generation reproduction rates, *19*, 133
of Australian aborigines, *12*, 498
* of early man, *5*, 35
of families of college students, *3*, 343
of professional persons, *12*, 199
of social classes, *5*, 256; *6*, 595; *12*, 199
* of the social classes in the native white population of Columbus and Syracuse, *6*, 595
rates, for U. S. white women, *12*, 42
secular drift in, *3*, 475
Fetal movements and maternal activity, *18*, 221
skulls, measurements of pituitary fossa in, *20*, 3
Field work in constitution study, *1*, 51
Finger and palm prints, compared, *10*, 14
and toe patterns, *8*, 531
and toe tip volume, *13*, 526
patterns, distribution of, *15*, 48
prints, *see also* Dermatoglyphics
of Bedouins, *9*, 358
of Indians and Spanish-Americans, *13*, 177
of triplets, *5*, 8
tip patterns, symmetry of, *10*, 77
volume and surface area, *11*, 536
of bone and soft tissue, *10*, 295
First births, Registration Area, 1930, *9*, 67
First-born, handicapping of, *1*, 382
*First menstruation and subsequent cycle of pubertal girls, *6*, 431
Fissiparous propagation, mathematical treatment of, *8*, 126
Fitting straight lines, effects of errors in one or both variables, *19*, 231
Flexion reflex, as reversion to primitive behavior, *1*, 409
*Folk medicine in Russia, *3*, 576
Food consumption, appetite levels of, *15*, 201
habits of California and Nevada aborigines, *15*, 165
likes of twins, *6*, 336
patterns, *15*, 202
Foot length, human, from embryo to adult, *16*, 207
of langurs and macaques, *14*, 457
of primates, *2*, 383
Foramen, nutrient, size and position, *11*, 369
Foramina, intervertebral, heights of, *16*, 164
*Forbes', John, *British and Foreign Medical Review* (1836-1847), and its relation to medical statistics, *6*, 637

Forbes, W. H., *see* McFarland, R. A.
Forearm extensors, phylogeny of, *13*, 23, 203
Foreign-born women, fertility of, *8*, 520
*Form discrimination test as given to Navajo, Negro and white school children, *13*, 239
Forsyth, C. H., Decline in Average Length of Life, *2*, 199-222
Fortuyn, A. B. Droogleever, Death — An Adaptation, *11*, 408-409
Foulger chart, for blood pressure score, *19*, 42
Four-fold table, historical note on, *20*, 161
Francis, Carl C., Growth of the Human Pituitary Fossa, *20*, 1-20
Fraternal relationship, and dermatoglyphics, *10*, 22
Freedom of the will, *2*, 471
Freeman, Bettie C., Fertility and Longevity in Married Women Dying after the End of the Reproductive Period, *7*, 392-418
Freeman, Walter, Biometrical Studies in Psychiatry: VII. Tuberculosis, Syphilis and Cancer, *4*, 208-238; VIII. The Weight of the Endocrine Glands, *6*, 489-523
Reversion to Primitive Behavior Resulting from Organic Disease of the Brain, *1*, 406-420
Freeman, Walter, *see also* Pearl, Raymond
Frequency distribution, logarithmic, *13*, 1
of monozygotic and dizygotic twin deliveries, *12*, 105
Freshmen, families of, *3*, 342
Friedenwald, J. S., Population Growth in Palestine, *9*, 347-356
Frost, Wade Hampton, portrait of, *10*, facing page 327
Fructosuria, inheritance of, *13*, 58
Fukuoka, Goro, *see* Komai, Taku
*Function of the arms in walking, *11*, 529
Functional imbalances and biophysical factors, *10*, 93
* theories of primate evolution, *2*, 223
*Further evidences of reversal in the human growth tide, *13*, 363
* note on the death rate among old-age assistance recipients in Rhode Island, *15*, 175
* observations on color blindness among Negroes, with genealogic and geographic notes, *8*, 451
* studies on the role of irritation in cancer, *9*, 57

Gaboon, gorillas from, *15*, 322
Gafafer, William M., Disease of the Upper Respiratory Tract (Common Cold) in Jews and Non-Jews, *4*, 429-433
John Forbes' *British and Foreign Medical Review* (1836-1847) and Its Relation to Medical Statistics, *6*, 637-649
Joseph Glanvill, a Forgotten Seventeenth Century Iconoclast, *4*, 121-130
Kenelm Digby, Seventeenth Century Psychotherapist, *5*, 506-515
The Inhabitants of the Island of St. Kilda and Acute Upper Respiratory Disease, *3*, 437-441
*Gain in weight and its association with ancestry and economic status, *12*, 532

* and the child's physical fitness, *15*, 295
Gallbladder, diseases of, and sex, *2*, 73
Gamble, Clarence J., *see* BEEBE, GILBERT W.
Games preferred by twins, *6*, 340
GARRETT, HENRY E., Mean differences and individual differences, *15*, 166-170
Gastro-intestinal disease, and sex, *2*, 63
Geisler, Murray A., *see* BEEBE, GILBERT W.
Gene frequency method, *7*, 608
*Genealogical methods as basis of research in human heredity, *2*, 250
General paralysis in Negroes, *7*, 492
Generation reproduction rates, *19*, 133
Generations, spread of, *1*, 305
Genetic(s) factors of longevity, *11*, 7
producing twinning, *11*, 179
records in constitution study, *1*, 19
* refutation of the principles of "behavioristic" psychology (Studies in human inheritance, VI), *3*, 547
of cleidocranial dysostosis, *18*, 103
of Rh blood factor, *17*, 195
traits, frequencies in Gentile and Jewish students, *16*, 172
Genital swelling in chimpanzee, *11*, 94
Genitalia of the simian primates, *8*, 309
*Geographic and time variations in body growth and age at menarche, *9*, 43
Geographical differences in mortality sex ratio, *1*, 100
distribution of Negroes, *4*, 4
regions of Mexico, in 1793, *14*, 502
Geography and vital phenomena in Korea, *3*, 331
Georgia mental patients, *11*, 513
Gestation month, sex ratio of abortions by, *20*, 157
period, whites and Negroes, *17*, 229
Gibbons, allometry in, *11*, 380
family of, *5*, 288
growth and classification of, *5*, 212, 385
GINI, CORRADO, Beauty, Marriage and Fertility, *10*, 575-576
GLADWIN, THOMAS, The Cranial Index: A Statistical Study, *13*, 88-102
*Glanvill, Joseph, a forgotten seventeenth century iconoclast, *4*, 121
GLANVILLE, A. DOUGLAS, and KREEZER, GEORGE, The Maximum Amplitude and Velocity of Joint Movements in Normal Male Human Adults, *9*, 197-212
Goiter and birth rate, *5*, 516
sporadic, inheritance of, *5*, 480
GOLDSTEIN, MARCUS S., Development of the Head in the Same Individuals, *11*, 197-219
and STANTON, FREDERICK L., Antero-Posterior Movements of the Teeth between Two and Ten Years, *8*, 161-197
Development of the Alveolar Arches in Normal and Abnormal Occlusion, *10*, 327-355
Gompertz's law, *11*, 4

Gonad weight, and mental disease, *7*, 571
Goniometer, use of, for joint movements, *9*, 200
Gooch, Marjorie, *see* PEARL, RAYMOND
*Good and poor years for the growth of girls in stature and body weight, *12*, 280
Goodwin, J. C., *see* CATES, H. A.
Gorilla, allometry in, *11*, 380
behavior in captivity, *9*, 175
hair tracts of, *12*, 206
skeletal and dental development, *15*, 236, 307
skeleton, skeletal indices, *16*, 75
GOULD, HARLEY N., The Physique of Women Students at Newcomb College of Tulane University. IV. Stature, Weight and Body Build by National Extraction, *5*, 122-129; V. Increase in Stature at College, Based on Repeated Measurements of the Same Individual, *11*, 220-233
Gould, Sophia, *see* PEARL, RAYMOND
GOVER, MARY, Increase of Negro Population in the United States, *1*, 263-273
GOWEN, JOHN W., Anomalous Human Sex-linked Inheritance of Color-blindness in Relation to Attached Sex Chromosomes, *5*, 130-134
Grandparents, logical classes of, *6*, 184
Graphic exposition, in biological and medical literature, *17*, 314
* method for representing the significance of the difference between means, *7*, 267
Grasping reflex, *1*, 415
GRAY, H., Body-Build and Diagnosis of Schizophrenia, *11*, 357-368
Individual Growth-Rates: From Birth to Maturity for 15 Physical Traits, *13*, 306-333
Gray H., *see also* BAYER, LEONA M.
Greece, ancient, ethnic groups in, *18*, 1
modern, mortality in, *8*, 553
Greek cranial measures, ancient, *18*, 6
* teeth: ancient and modern, *16*, 283
Greene, Elizabeth, *see* COHEN, B. M.
GREENE, J. E., and PHILLIPS, W. S., Racial and Regional Differences in Standard Therapy Rates among White and Negro Mental Patients, *11*, 513-528
GREENWOOD, MAJOR, "Nerves" and the Public Health, *4*, 155-178
and IRWIN, J. O., The Biostatistics of Senility, *11*, 1-23
GREGORY, WILLIAM K., Critique of Professor Wood-Jones' Paper, *2*, 99-108
Is the Pro-Dawn Man a Myth? *1*, 153-165
Polyisomerism and Anthropogeny, *6*, 632-636
Group algebra, *19*, 186
Groups, comparison of measurements of, *7*, 95
social, quantitative aspects of, *19*, 163
*Growth, a third aspect of, *6*, 405
acceleration, *4*, 308
in adolescence, *7*, 330; *15*, 14
and development, *1*, 128
and menarcheal age, *9*, 43
* changes in physical correlation, *1*, 462

* in the cephalic index, during the first five years of life, *8*, 1
* classification, and evolutionary specialization of gibbons and siamangs, *5*, 212, 385
constants, *9*, 559
of man, *10*, 210
correlations in children, *19*, 260
curves for juvenile adults, *11*, 322
* of nations, *18*, 204
of Negro and white populations, *1*, 263
of physical abilities, by sex, *19*, 12
date of birth and, *7*, 303
equation, *9*, 551, 557; *10*, 147, 185; *15*, 1
facial, in children, *20*, 109
* formula of Bertalanffy and its similarity to equations for excitation and recovery in the central nervous system, *11*, 402
human, evidence of trends in, *10*, 548; *13*, 363
seasonal cycle in, *2*, 523
* in body weight during the first ten days of postnatal life, *11*, 25
in limb circumference, *9*, 398
in skull dimensions, *14*, 143
individual, of school children, *7*, 319
neonatal, and birth weight, *11*, 24
of children, *4*, 307; *5*, 428; *9*, 567; *12*, 280
Porto Rican, *4*, 469
of college women, *11*, 220
of eyeball, *12*, 11
of infants with eczema, *11*, 269
of limb bones of anthropoids, *11*, 379
of macaques, *13*, 282
of population, *8*, 25, 399
in Washington County, Maryland, *12*, 64
of the extremities, *9*, 366
* of the human pituitary fossa, *20*, 1
of white mouse and white rat, *10*, 198
organic, quantitative theory of, *10*, 181
* patterns of the human physique: an approach to kinetic anthropometry, *15*, 1
rate(s), age at maximum, *4*, 308
individual, for 15 physical traits, *13*, 306
of population, correlation with vital factors, *3*, 180
relationship of individual to average, *3*, 37
school children of two socio-economic groups, *19*, 68
sex differences in human, *15*, 15
strength and weight by sex, *19*, 12

GUMBEL, E. J., Probability of Death and Expectation of Life, *18*, 238-240

GUNSTAD, BORGHILD, and TRELOAR, ALAN E., The Relationship between the Length and Weight of the New-born Infant and the Height and Weight of the Mother, *8*, 565-580

Habitus, bodily, of cardiacs and non-cardiacs, *6*, 699
drepanocytanemic, *16*, 111
* of patients with sickle cell anemia, *16*, 101
type, distributions of, in schizophrenics, *14*, 196
Hair-color, and physical types, *13*, 541
Hair, density of, in primates, *3*, 303
thigh and chest, *17*, 141
of gibbons, *5*, 250
pubic, sex differences in distribution, *17*, 137
texture zones, *2*, 40
tracts, primate, prenatal development of, *12*, 203
whorl, in twins, *6*, 28
HALDANE, J. B. S., The Blood-Group Frequencies of European Peoples, and Racial Origins, *12*, 457-480
Hall, F. G., *see* ROBINSON, S.
Hand(s), monomorphic, *10*, 468
of langurs and macaques, *14*, 457
of primates, *2*, 381
ossification of, *6*, 454
* size in working women, *8*, 267
*Handedness and dermatoglyphics in twins, *15*, 46
and hemihypertrophy, *11*, 457
diagnosis of, *14*, 518
frequencies among Gentile and Jewish students, *16*, 177
among Japanese school children, *6*, 33
in twins, *6*, 25
sex differences, *19*, 21
HARDY, MARTHA C., Anthropometric Studies from McCormick Fund, *10*, 285-294
Harelip, laterality of occurrence, *11*, 466
Harmon, P. M., *see* ROBINSON, S.
HARRIS, DALE B., and HARRIS, ELIZABETH S., A Study of Fetal Movements in Relation to Mother's Activity, *18*, 221-237
Harris, Elizabeth S., *see* HARRIS, DALE B.
HARVEY, O. L., and CROCKETT, HAZEL E., Individual Differences in Temperature Changes of Women during the Course of the Menstrual Cycle, *4*, 453-468
*Harvey's post-mortem examination of Thomas Parr, *3*, 138
Hatch, C. E., *see* HOLMES, S. J.
Head development, *8*, 6; *11*, 197
di- nsions, of different disease groups, *6*, 689; *8*, 67
of metopic individuals, *17*, 114
of Negroes, *15*, 130
of neonates, *8*, 440
of schizophrenics, *11*, 357
of women, *14*, 44
sex differences in, *15*, 145
Headlong Hall, *1*, 139
*Health, depressions, weather and, *10*, 388

of twins, *3*, 270
Hearing in siblings and non-siblings, *11*, 259
Heart defects, congenital, *1*, 396
disease(s) of, and constitution, *6*, 650; *8*, 38
and height-weight, *1*, 166
and weight, *2*, 167
mortality, and country of origin, *17*, 348
of Negroes, *3*, 225
measurements of cardiacs and non-cardiacs, *8*, 46, 57
rate, and birth order in children, *15*, 171
during pregnancy, *8*, 233
in moderate and maximal work, *13*, 142
Heel contour, first delineation of, *16*, 213
Height(s), *see also* Body length, Stature
and season of birth, *13*, 531
and specific gravity, *5*, 658
* and weights of American college men, *5*, 445
* of American college women, *5*, 122, 600
of infants, *10*, 409
* of Ottawa elementary school children of two socio-economic strata, *19*, 68
of schizophrenics, *11*, 357
correlated with increase in height, *19*, 260
growth in, *1*, 462
of college women, *11*, 220
of intervertebral foramina, *16*, 167
of Japanese women, *14*, 288
of pubescent and non-pubescent girls, *9*, 26
sitting, and vertex height, of Eurafrican juveniles, *11*, 326
-weight changes of college freshmen, *13*, 367
index, growth pattern of, *15*, 5
standards, *1*, 190
Hemiatrophy and hemihypertrophy, congenital, *11*, 447
Hemoglobin, hourly variations in, *10*, 511
in pregnancy, *10*, 534
variation with age in infants, *10*, 139
HENCKEL, K. O., Remarks on the Papers by Professor F. Wood-Jones and Professor W. K. Gregory concerning the Phylogeny of Primates, *2*, 555-557
Herbert, Kitty Gale, *see* BOYNTON, PAUL L.
*Hereditary mental defect, physique in, *13*, 459
tendency to eliminate upper lateral incisors, *4*, 34
Heredity and the variability of the lateral incisors, *12*, 345
influence of, in causation of disease, *17*, 362
on duration of life, *4*, 41
of cephalic index in Holland, *5*, 587
* of the agglutinogens M and N of Landsteiner and Levine. IV. Additional theoretico-statistical considerations, *7*, 222
* of the subgroups of group A and group AB_1, *5*, 577

(Heredity)
of twins as factor in differences in environment, *6*, 324
Heritage and habitus, *2*, 101
HERSKOVITS, MELVILLE J., Physical Types of West African Negroes, *9*, 483-497
Population Statistics in the Kingdom of Dahomey, *4*, 252-261
Social Selection and the Formation of Human Types, *1*, 250-262
HESS, LEO, Ossicula Wormiana, *18*, 61-80
The Metopic Suture and the Metopic Syndrome, *17*, 107-136
*Hiatus canalis sacralis in American whites and Negroes, *17*, 368
HIMES, NORMAN E., *Marriage Hygiene*: A Scientific Quarterly, *7*, 119-120
*Historical error corrected, *9*, 104
*History of a gesture, *2*, 566
* of longevity in the United States, *6*, 43
Holland, Dorothy F., *see* WINSLOW, C. -E. A.
Holland, inheritance of cephalic index in, *5*, 587
HOLMES, S. J., Differential Mortality in the American Negro, *3*, 71-106, 203-244
and HATCH, C. E., Personal Appearance as Related to Scholastic Records and Marriage Selection in College Women, *10*, 65-76
and MENTZER, V. P., Changes in Sex Ratio in Infant Mortality According to Age, *3*, 560-575
Home environment records in constitution study, *1*, 45
ownership, and permanence of residence, *19*, 100
*Homicide in New York State, *7*, 47
rate, *1*, 274
and church membership, *1*, 562
Homogamic correlation of longevity, *6*, 199
*Homogamy and infertility, *8*, 19
in man, *5*, 690
Homosexuals, physiological and psychological patterns of, *9*, 510
HOOTON, EARNEST A., Doubts and Suspicions concerning Certain Functional Theories of Primate Evolution, *2*, 223-249
HOPKINS, J. W., Height and Weight of Ottawa Elementary School Children of Two Socio-economic Strata, *19*, 68-82
HOSOI, KIYOSHI, and ALVAREZ, WALTER C., Influence of Sex on Incidence of Gastro-intestinal Disease, *2*, 63-98
Hospitalization facilities in Georgia, *11*, 516
*Hourly physiologic variations in peripheral hemoglobin, red and white cell counts of women: Effect of pregnancy upon blood counts, *10*, 511
HOWELLS, W. W., Some Uses of the Standard Deviation in Anthropometry, *8*, 592-600
Hsü, E. H., *see* MOORE, T. V.
HUBER, EDWARD GODFREY, Systolic and Diastolic Blood Pressure in Healthy Men, *5*, 541-576
HUFF, C. ELTON, and PYLE, S. IDELL, Differences in the Utilization of Calcium and Phosphorus in Negative and Positive Balances during Pregnancy, *9*, 29-43
Hughson, W., *see* CIOCCO, ANTONIO
*Human abortion, *6*, 271
biology in Korea, *3*, 322

* finger tip: surface area and volume correlations, *11*, 536
* foot length from embryo to adult, *16*, 207
* sacrifice and warfare as factors in the demography of pre-colonial Mexico, *18*, 81
* social biology. II. Disruptive and cohesive factors in the marital group, *10*, 555; III. Elements affecting the formation of the marital group, *11*, 234
types, formation of, *1*, 250
Humerus of primates, *2*, 360
Hunscher, Helen A., *see* KELLY, HARRIET J.
*Hunter, John, on appetites and aging, *1*, 565
Hunting populations, density of, *5*, 38
Hybridization, rate of, in California Indians, *15*, 161
Hylobates, *see* Gibbons
*Hypnotizability (Studies in Personality, A), *3*, 1
Hypophysis, weight of, *6*, 494; *7*, 565
Hypothenar dermatoglyphics, *7*, 1
Hysteria, and hypnotizability, *3*, 1, 23

Iconometrography in graphic exposition, *17*, 314
Identical triplets, *5*, 1
twins, *see* Twins
Idio-savants, *3*, 548
Ill-defined causes, deaths from, in Negroes, *3*, 235
Illegitimacy, and sex ratio, *10*, 49
Illiteracy and the churches, 4, 422
Imbalances, functional, and biophysical factors, *10*, 93
Immigration, and population growth, *8*, 25
effect of, on logistic population, *3*, 487
Impressionistic somatological typing, *14*, 27
Inbreeding and fecundity, *5*, 57
and intelligence, *20*, 171
in study of heredity, *2*, 254
Incisors, upper lateral, hereditary tendency to eliminate, *4*, 34
Increase in stature of women at college, *11*, 221
* of Negro population in United States, *1*, 263
Increment of alveolar arches, *10*, 340
Index (indices), anthropometric, of cardiacs and non-cardiacs, *6*, 695
of body build, *15*, 127
of physical type, *3*, 420
thoracic, *6*, 1
Indians, American, crania with low vaults, *14*, 178
deaths by age, *3*, 397; *7*, 335; *8*, 223; *19*, 85
dermatoglyphics of, *13*, 177
homosexuality among, *9*, 498
human sacrifice and warfare, *18*, 8
migration of, *15*, 33
mortality and morbidity, *3*, 397

(Indians)
of California, census of, *15*, 34
performance on form discrimination test, *13*, 240
racial fusion among, *15*, 153
*Individual differences in temperature changes of women during the course of the menstrual cycle, *4*, 453
* mean differences and, *15*, 166
* growth rates: from birth to maturity for 15 physical traits, *13*, 306
Industrial applications of spread of generations, *1*, 317
preventive medicine, *19*, 27
psychology, *4*, 176
Infant(s), bodily and gastric activity of the new-born, *8*, 368, 381
body build in newborn, *6*, 612; *8*, 433
in, with eczema, *11*, 269
mortality, correlation with growth-rate of population, *3*, 187
in New York City, *5*, 98
influence of public health procedures on, *9*, 133
Negroes, *3*, 235; *4*, 1
rate, logarithmic frequency distribution of, *13*, 3
San Juan pueblo, *12*, 170
seasonal variations in, *4*, 19
sex factor in, *1*, 90; *4*, 17
ratio in, *3*, 560
-mother measurements, *8*, 565
* Negro, *4*, 1
Infections of digestive tract, and sex, *2*, 67
Infertility and homogamy, *8*, 19
in U. S. white women, *12*, 42
*Influence of abnormal sitting height indices on weight and lung volume predictions (Vital function studies, XIII), *7*, 240
* of certain public health procedures upon infant mortality, *9*, 133
* of heredity upon the duration of life in man, based on a Chinese genealogy from 1365 to 1914, *4*, 41
* of sex on incidence of gastro-intestinal disease, *2*, 63
* of weight on causes of death, *2*, 159
Influenza, and weight, *2*, 179
in Negroes, *3*, 212
in Negro infants, *4*, 25
Information service for research workers in human genetics, *11*, 288
*Inhabitants of the island of St. Kilda, and acute upper respiratory disease, *3*, 437
Inheritance and severity of disease, *4*, 69
blood group, *17*, 13
cardiovascular disease, *13*, 350
of cephalic index, *5*, 587
* of cleidocranial dysostosis, *18*, 103
of color-blindness, *5*, 130
of fructosuria, *13*, 58
* of longevity, *6*, 98

* (Studies on human longevity, IV), *3*, 245
* of l-xyloketosuria (essential pentosuria), *8*, 243
* of pigmentation in the American Negro, *1*, 321
of Rh blood factor, *17*, 195
* of sporadic goiter, *5*, 480
* studies in human. XII. Application of the gene frequency method of analysis to sex influenced factors, with especial reference to baldness, *7*, 608
*Inherited reductions in the dentition of man, *6*, 627
Injury, secondary effects of, *1*, 120
*Inquiry into methods of studying the sex ratio for the United States during war and post-war years, *15*, 255
on the "Safe Period," *9*, 251
Inquisitions Post Mortem, *9*, 528
*Institutionalized homosexuality of the Mohave Indians, *9*, 498
Intelligence and fecundity, *3*, 348
and fertility, *12*, 114; *13*, 410
and hypnotizability, *3*, 34
and inbreeding, *20*, 171
* quotient and physical quotient, *2*, 473
and vital indices, *5*, 94
constancy of, *2*, 499
relation to physical measurements, *14*, 527
two aspects of, *2*, 470
Inter-cristal width, growth in, *7*, 164
*Interdependence of the length of life of husband and wife, *13*, 505
Intergroup anatomic lag, *6*, 533
Intermarriage rate in New York State, *3*, 379
Intermediate type, in schizophrenic women, *14*, 206
Intermembral index, in primates, *2*, 368
Internal environment and neuropathology, *7*, 74
International Union for the Scientific Investigation of Population Problems, *6*, 223
*Interrelations of hemoglobin, basal metabolism, and creatine, creatinine and magnesium excretion during human pregnancy, *10*, 528
Interrelationships of habitus types, *14*, 224
*Interval between successive births and its effect on survival of infant, I. An indirect method of study, *17*, 65
*Intracranial variation in the weight of the human brain, *14*, 48
Intra-uterine mortality, and sex ratio, *10*, 53
*Introduction of blood-group B into Europe, *14*, 413
Iowa, female population of, *7*, 514
Irritation in cancer, *9*, 57
Irwin, J. O., *see* GREENWOOD, MAJOR
*Is man a rational animal? *2*, 457
* the ape-man a myth? *1*, 4
* the human growth tide turning? *10*, 548
* the pro-dawn man a myth? *1*, 153

ISBELL, H., The Human Finger Tip; Surface Area and Volume Correlations, *11*, 536-542
Ischium-pubis index, in langurs and macaques, *14*, 454
Isolation, effect on racial fusion, *15*, 155
Italians in New York City, *5*, 118
ITO, PAUL K., Comparative Biometrical Study of Physique of Japanese Women Born and Reared under Different Environments, *14*, 279-351

Jackson, Nelle E., *see* WHELPTON, P. K.
JAFFE, A. J., Notes on the Rate of Growth of the Chinese Population, *19*, 1-11
and LOURIE, W. I., JR., An Abridged Life Table for the White Population of the United States in 1830, *14*, 352-371
JANER, JOSÉ L., Population Growth in Puerto Rico and Its Relation to Time Changes in Vital Statistics, *17*, 267-313
Japan, paternity in, *2*, 508
population problem in, *4*, 408
resources, *4*, 408
vital indices, *1*, 198
Japanese women, physiques of, *14*, 279
JELLINEK, MORTON, Estimates of Intra-individual and Inter-individual Variation of the Erythrocyte and Leukocyte Counts in Man, *8*, 581-591
JENSS, RACHEL M., An Inquiry into Methods of Studying the Sex Ratio at Birth for the United States during War and Post-war Years, *15*, 255-266
Gain in Weight and Its Association with Ancestry and Economic Status, *12*, 532-544
Gain in Weight and the Child's Physical Fitness, *15*, 295-306
and BAYLEY, NANCY, A Mathematical Method for Studying the Growth of a Child, *9*, 556-563
Jews, common cold among, *4*, 429
in New York City, *5*, 118
personality disorders among, *11*, 118, 486
population growth, *9*, 349
Joachimsthal, lung cancer among mine workers in, *11*, 130
Johnston, A. Theresa, *see* KELLY, HARRIET J.
Joint movements in adults, *9*, 197
JONES, HAROLD E., Sex Differences in Physical Abilities, *19*, 12-25
Jones, Harold E., *see also* BURKS, BARBARA S.
see also WILSON, PAUL T.
JUNG, FREDERIC T., A Statistical Phenomenon Involving Hair-Color and Physical Types, *13*, 541-545
Juvenile delinquency, and vital indices, *5*, 94

KANNER, LEO, Names of the Falling Sickness, *2*, 109-127
KARPINOS, BERNARD D., A Stationary Population (A Study of Iowa Population), *7*, 514-538
Prevalence of Pulmonary Tuberculosis among Selectees, *16*, 181-189
Use of Life Table Death Rates for Comparative Mortality, *18*, 127-131

Visual Acuity of Selectees and Army Inductees, *16*, 1-14
KÄSSBACHER, MAX, Genealogical Methods as Basis of Research in Human Heredity, *2*, 250-263
Proposal for the Compulsory Anthropological Study of Emigrants, *2*, 557-558
KELLER, J. D., Growth Curves of Nations, *18*, 204-220
KELLY, HARRIET J., SOUDERS, HELEN J., JOHNSTON, A. THERESA, BOUND, LOUISE EMERSON, HUNSCHER, HELEN A., and MACY, ICIE G., Daily Decreases in the Body Total and Stem Lengths of Normal Children, *15*, 65-72
KEMP, TAGE, The Inheritance of Sporadic Goiter, *5*, 480-490
KENYON, FANNY, and MACY, ICIE G., Hourly Physiologic Variations in Peripheral Hemoglobin, Red and White Cell Counts of Women: Effect of Pregnancy upon Blood Counts, *10*, 511-527
Kidneys and related excretory organs in old age, *4*, 91, 105
Kinetics of sulfonamide absorption and elemination, *18*, 171
KING, MARGUERITE N., Cultural Aspects of Birth Control in Puerto Rico, *20*, 21-35
Kinship correlation, of longevity, *6*, 198, 204
KIRKPATRICK, CLIFFORD, and CEDARSTRAND, GRACE, Nature and Nurture in Relation to Twins and Brothers in *Who's Who*, *5*, 371-384
KISER, CLYDE V., Trends in the Fertility of Social Classes from 1900 to 1910, *5*, 256-273
Kiser, Clyde V., *see* NOTESTEIN, FRANK W.
KISH, JOSEPH F., The Quartile Abscissa of the Normal Curve to 15 Places of Decimals, *11*, 285-287
The Standard Error of the Slope of a Straight Line, *12*, 264-279
KOMAI, TAKU, and FUKUOKA, GORÔ, A Note on the Problem of Mirror-imaging in Human Twins, *6*, 24-32
A Study on the Frequency of Left-handedness and Left-footedness among Japanese School Children, *6*, 33-42
Korea, human biology in, *3*, 322
Kreezer, George, *see* GLANVILLE, A. DOUGLAS
Kretschmerian typology, *9*, 451; *14*, 24
KROGMAN, WILTON MARION, A Study of Four Skulls from Seleucia-on-the-Tigris Dating from 100 B. C. to 200 A. D., *12*, 313-322
Vital Data on the Population of the Seminole Indians of Florida and Oklahoma, *7*, 335-349

Lag, anatomic, *6*, 524; *13*, 473
DE LAGUNA, GRACE A., Perception and Language, *1*, 555-558
Land areas and populations, *12*, 361
distribution of, and racial distribution, *2*, 37
LANDAUER, W., Supernumerary Nipples, Congenital Hemihypertrophy and Congenital Hemiatrophy, *11*, 447-472
LANDIS, CARNEY, BOLLES, MARJORIE, and D'ESOPO, D. A., Psychological and Physical Concomitants of Adjustment in Marriage, *12*, 559-565
Language and perception, *1*, 555
Langurs, skeletal proportions, *14*, 444
Lanier, Patricia F., *see* TROTTER, MILDRED

LASKER, GABRIEL W., Penetrance, Estimated by the Frequency of Unilateral Occurrences and by Discordance in Monozygotic Twins (With an Editorial Note by Charles P. Winsor), *19*, 217-230
The Inheritance of Cleidocranial Dysostosis, *18*, 103-126
and REYNOLDS, H. H., A Cicatrix of the Neck Associated with Anomalous Dental Occlusion in One of a Pair of Monozygous Twins, *20*, 36-46
Lasker, Gabriel W., *see also* LASKER, MARGARET
LASKER, MARGARET, Essential Fructosuria, *13*, 51-63
ENKLEWITZ, MORRIS, and LASKER, GABRIEL W., The Inheritance of L-Xyloketosuria (Essential Pentosuria), *8*, 243-255
*Lateral incisor teeth, significance of variability of, *12*, 323
* vertebral dimensions and lateral spinal curvature, *16*, 153
Laterality as a measure of penetrance, *19*, 217
Laughing, pathologic, *1*, 417
*Law(s) of evolution as a maximal principle, *17*, 167
of mortality, at old ages, *11*, 13
in *Drosophila*, *11*, 5
of serological race classification, *2*, 128
of small numbers, *19*, 154
LAWRENCE, PHILIP S., Ancestral Longevity and Sex Ratio of the Descendants, *12*, 403-429
Least squares lines, various criteria for, *19*, 232
Lebistes reticulatus, length and weight growth of, *10*, 188
LEBLANC, THOMAS J., Sidelights on the Population Problem in Japan, *4*, 408-420
Some Aspects of Paternity in Sendai, Japan, *2*, 508-522
Some Phases of Human Biology in Korea (Chosen), *3*, 322-341
Specific Vital Indices for Japan, *1*, 198-213
Leg girth, growth of, *9*, 384
Length, formula for growth in, *9*, 557
of humerus and of femur in primates, *9*, 290
of life, *see also* Longevity
decline in, *2*, 199
in England, 1250-1348, *9*, 528
Lethal effects of sexual continence, *3*, 577
Leukocyte counts in man, intra- and inter-individual variation, *8*, 581
LEVIN, SAMUEL M., Malthus' Conception of the Checks to Population, *10*, 214
Lexis' theory of mortality, *11*, 7
LIENAU, C. C., Quantitative Aspects of Organization, *19*, 163-216
*Life tables, abridged, for white population of the United States in 1830, *14*, 352
* current versus generation, *13*, 475
death rates of, use of for comparative mortality, *18*, 127, 238
Drosophila, *11*, 5
English, *11*, 10
* for a southern Chinese family, *3*, 157
generation, England and Wales, *13*, 442
graduations of, Gompertz' and Lexis' methods, *11*, 12
hypothetical, *6*, 81

methods in closed population groups, *18*, 158
New York City, 1805-1808, *6*, 90
Parsi in Bombay, *20*, 74
Puerto Rico, *17*, 282
San Juan pueblo Indians, *12*, 180
United States, development of, *6*, 46
Light stimulation, and activity of infants, *8*, 381
*Like-sexed twins, early mortality of, *12*, 247
Limb bones of anthropoids, relative growth of, *11*, 379
of gibbons, *5*, 248
LINDER, FORREST E., and CARMICHAEL, HUGH T., A Biometric Study of the Relation between Oral and Rectal Temperatures in Normal and Schizophrenic Subjects, *7*, 24-46
Linked factors, misconception concerning, *11*, 546
Lipson, Henry I., *see* SCHREK, ROBERT
Liver, diseases of, and sex, *2*, 73
*Logarithmic correlation coefficients and regression equations, *14*, 95
* frequency distributions, *13*, 1
*Logistic curve(s), and calculus of variations, *11*, 173
and population structure, *3*, 459
describing population growth, Puerto Rico, *17*, 271
development of theory of, *10*, 1
immigration and, *8*, 25
mean of several, *3*, 43
population, birth rate and density in, *4*, 119
use of to represent prevalence of defective vision, *7*, 257
Verhulst and, *5*, 673
world population, *8*, 408
LOMBARD, HERBERT L., TULLY, MILDRED R., and POTTER, EVELYN A., Evaluation of the Cancer Educational Program in Massachusetts, *16*, 115-125
Long bones in man and apes, *9*, 281
LONGCOPE, WARFIELD T., The Sick Man, *1*, 445-461
*Longevity, a pedigree, *3*, 133
* ancestral, and sex ratio of descendants, *12*, 403
and fertility, *7*, 392
constitutional factors in mortality, *4*, 80
correlation between husbands and wives, *12*, 512; *13*, 189, 505
heredity and, *4*, 41
* history of, in United States, *6*, 43
inheritance of, *3*, 245; *6*, 98
of Negro, and environmental changes, *17*, 224
LORIMER, FRANK, Eugenics Research Survey, *3*, 288
LOTKA, ALFRED J., A Historical Error Corrected, *9*, 104-107
Spread of Generations, *1*, 305-320
The Law of Evolution as a Maximal Principle, *17*, 167-194
The Structure of a Growing Population, *3*, 459-493
Lotka, A. J., *see also* DUBLIN, LOUIS I.

LOVE, ALBERT G., Somatological Norms in Tuberculosis and Heart Disease, *1*, 166-197
Love, Albert G., *see* REED, LOWELL J.
Lower classes, are they inferior? *1*, 509
LUMER, HYMAN, Relative Growth of the Limb Bones in the Anthropoid Apes, *11*, 379-392
and SCHULTZ, ADOLPH H., Relative Growth of the Limb Segments and Tail in *Ateles geoffroyi* and *Cebus capucinus*, *19*, 53-67
Relative Growth of the Limb Segments and Tail in Macaques, *13*, 283-305
LUND, S. E. TORSTEN, A Psycho-biological Study of a Set of Identical Girl Triplets, *5*, 1-34
*Lung cancer among mine workers in Joachimsthal, *11*, 130
ventilation in moderate and maximal work, *13*, 148
volume, prediction of, and sitting height index, *7*, 251
LUYKX, H. M. C., Family Studies in the Eastern Health District. IV. Permanence of Residence with Respect to Various Family Characteristics, *19*, 91-132
L-xyloketosuria, the inheritance of, *8*, 243
Lyman, R. S., *see* STEVENSON, P. H.

Macaques, prenatal and postnatal growth, *13*, 287
skeletal proportions, *14*, 444
MACARTHUR, JOHN W., Reliability of Dermatoglyphics in Twin Diagnosis, *10*, 12-35
and MCCULLOUGH, ERNEST, Apical Dystrophy: an Inherited Defect of Hands and Feet, *4*, 179-207
MacKinnon, Donald W., *see* BARRY, HERBERT, JR.
MACKLIN, MADGE THURLOW, Primogeniture and Developmental Anomalies, *1*, 382-405
The Relation of the Mode of Inheritance to the Severity of an Inherited Disease, *4*, 69-79
Macy, Icie G., *see* KELLY, HARRIET J.
see also KENYON, FANNY
Malaria in Negroes, *3*, 90
library, *6*, 240
Malformations in Negroes, *3*, 232
of the digestive tract, congenital, and sex, *2*, 67
MALLER, J. B., Vital Indices and Their Relation to Psychological and Social Factors, *5*, 94-121
Malocclusion, alveolar arches and, *10*, 339
*Malthus' conception of the checks to population, *10*, 214
MALZBERG, BENJAMIN, Mental Disease among Negroes in New York State, *7*, 471-513
Mortality among Patients with Paranoia or Paranoid Conditions, *8*, 601-606
Man, early, fecundity of, *5*, 35
is he a rational animal? *2*, 457
Mandible, development of, *10*, 340
Mandibular area of primates, hair tracts of, *12*, 211

diastemata in the great apes, *12*, 331
measurements and indices of ancient skulls, *12*, 318
Manic-depressive psychoses, *11*, 358
in Negroes, *7*, 500
Maori blood groups, *3*, 282
Marasmus in Negroes, *3*, 233
Marital condition, correlation with growth-rate of population, *3*, 197
conflict, elements of, *10*, 569
group, elements affecting formation of, *11*, 234
intercourse, first reaction, *12*, 562
longevity, *13*, 505
* status, cancer and, *15*, 73
estimation of numbers by, *12*, 52
in Mexico in 1793, *14*, 507
Marriage(s), adjustment in, *12*, 559
age at, and duration of, in relation to conflict, *10*, 560
in Korea, *3*, 329
personal appearance, and neuroticism, *16*, 15
and personality, *7*, 430
* in the state of New York, with special reference to nativity, *3*, 376
restrictions on, in Bavaria, *4*, 286
MARTIN, W. S., Social Class Mortality, *17*, 1-12
*Masculinity of stillbirths and abortions in relation to the duration of uterogestation and to the stated causes of foetal mortality, *10*, 235
Masticatory morphology, in ancient and modern Greece, *16*, 286
Maternal activity and fetal movement, *18*, 221
age, and epilepsy, *18*, 49
*Mathematical method for studying the growth of a child, *9*, 556
theory of logistic curve, *11*, 177
Mathematics of growth, *9*, 557
Mating(s) pattern, of chimpanzee, *11*, 79
producing longevous persons, *6*, 209
season of primitive man, *5*, 49
Mauritius, population saturation in, *1*, 528
Maxillary area of primates, hair tracts in, *12*, 209
*Maximum amplitude and velocity of joint movements in normal male human adults, *9*, 197
Mayo Clinic, age and sex of patients at, *2*, 185
McCullough, Ernest, *see* MACARTHUR, J. W.
McFARLAND, R. A., and FORBES, W. H., The Metabolism of Alcohol in Man at High Altitudes, *8*, 387-398
McGRAW, MYRTLE B., Quantitative Behavior Data and the Longitudinal Method. The Moro Reflex, *9*, 542-548
MEAD, CHARLES H., A Quantitative Study in Human Teratology, *2*, 1-33
*Mean differences and individual differences, *15*, 166
Means, significance of difference of two series of, *6*, 402
Measles, in Negroes, *3*, 77; *4*, 24

Measurement(s), anthropometric, of Japanese women, *14*, 285
of patients with sickle cell anemia, sicklemia, *16*, 104
technique of, *1*, 41
calipers for cranial, *9*, 417
comparative bodily, of Mrs. Ruth Pontico, *14*, 170
cranial, American Indian, *14*, 180
growth in, *14*, 145
of langurs and macaques, *14*, 459
* of Western Reserve collection, *14*, 473
head circumference, *14*, 29, 529
of chest cross-section, method for, *2*, 539
of Eurafrican juveniles, *11*, 319
of female morons, *13*, 463
of new-born infant and of mother, *8*, 565
of pigmentation, technique of, *1*, 322
of triplets, *5*, 13
of twelve-day old babies, *8*, 439
of upper extremities of children, *11*, 278
* of variations in asymmetrical data with applications to hemoglobin statistics of infants, *10*, 136
of women with, and without infant mortality, *12*, 235
technique for cranial capacity, *14*, 473
for finger tip volume and surface area, *11*, 539
Medical specialization, benefits and dangers of, *1*, 447
*Medicine, biological philosophy and, *1*, 117
Medieval population, length of life in a, *9*, 528
Melanesians, racial composition of, *2*, 47
vital statistics of, *3*, 551
Menarcheal age and chest width, *8*, 217
and growth in strength, *19*, 19
and growth of head, *11*, 208
and maximum growth rate, *13*, 320
and physical development, *8*, 198; *9*, 1
and season of conception, *13*, 384
of Japanese women in different environments, *14*, 333
of Jewish girls, *6*, 433
of New York school girls, *4*, 308
of university girls, *13*, 365
racial and geographic differences in, *9*, 46, 80; *14*, 335
time trends in, *13*, 367
Menstrual cycle of Jewish girls, *6*, 438
temperature changes in, *4*, 453
* record during the menopause, *9*, 564
Menstruation, in chimpanzees, *11*, 86
*Mental and physical development as influenced by season of conception, *13*, 378
capacity, do races differ in? *1*, 70
defect, physique in hereditary, *13*, 459

diseases, among Negroes, in New York State, *7*, 471
and endocrine organs, *7*, 350, 555
somatology of, in Army officers, *5*, 89
disorders, whites and Negroes, in Georgia, *11*, 514
patients, age at death of, *7*, 354; *11*, 514
status and bodily habitus, *14*, 207
and season of conception, *14*, 251
Mentzer, V. P., *see* HOLMES, S. J.
Meredith, E. Matilda, *see* MEREDITH, HOWARD V.
MEREDITH, HOWARD V., Concerning a Recently Reported Finding on the Physical Growth of Boys, *9*, 567-571
Human Foot Length, from Embryo to Adult, *16*, 207-282
The Prediction of Stature of North European Males throughout the Elementary School Years, *8*, 279-283
and BOYNTON, BERNICE, The Transverse Growth of the Extremities: An Analysis of Girth Measurements for Arm, Forearm, Thigh and Leg Taken on Iowa City White Children, *9*, 366-403
and BROWN, A. W., Growth in Body Weight during the First Ten Days of Postnatal Life, *11*, 24-77
and MEREDITH, E. MATILDA, The Stature of Toronto Children Half a Century Ago and Today, *16*, 126-131
Merovingian crania, *6*, 309
MERRELL, MARGARET, Relationship of Individual to Average Growth, *3*, 37-70
Merrell, Margaret, *see also* PHAIR, JOHN J.
*Messance: French demographer, *12*, 77
Metabolism, in maximal and moderate work, *13*, 148
* of alcohol in man at high altitudes, *8*, 386
METHENY, ELEANOR, The Variability of the Percentage Index of Build as Applied to the Prediction of Normal Weight, *11*, 473-484
*Method of computing the cross-section of the chest, *2*, 539
*Methods (Studies on constitution, I), *1*, 10
*Metopic suture and the metopic syndrome, *17*, 107
Metopism, and ossicula wormiana, *18*, 61
Mexico, human sacrifice and warfare in pre-colonial, *18*, 81
population in 1793, *14*, 499
MICHELSON, NICHOLAS, Studies in the Physical Development. V. The Ossification Time of the Os Pisiforme, *17*, 143-146; VI. The Chronology of Physiologic Events, *17*, 147-150; VII. Environmental Trends among the American Negro, *17*, 207-228; VIII. Resumé and Goals for Further Research, *17*, 229-242
The Calcification of the First Costal Cartilage among White and Negroes, *6*, 543-557
MICHELSON, TRUMAN, Notes on Mongolian Craniology, *1*, 421-423
Microfilm copying, *12*, 566
sets of periodicals, *12*, 120
Mid-digital hair in Baghdad population, *13*, 400
*Migration(s) and urbanization of the Indians in California, *15*, 33
correlation with growth-rate of population, *3*, 188

(Migration)
from and to Puerto Rico, *17*, 279
rates, in Eastern Health District of Baltimore, *15*, 101; *19*, 91
zones, racial, *2*, 34; *13*, 390
MILES, MERYL, Lateral Vertebral Dimensions and Lateral Spinal Curvature, *16*, 153-171
MILLAR, WILLIAM M., Human Abortion, *6*, 271-307
MILLS, CLARENCE A., Depressions, Weather and Health, *10*, 388-399
Further Evidences of Reversal in the Human Growth Tide, *13*, 363-377
Geographic and Time Variations in Body Growth and Age at Menarche, *9*, 43-57
Mental and Physical Development as Influenced by Season of Conception, *13*, 378-389
Mental Status and Season of Conception, A Reply to the Criticism of Malcolm G. Preston, *14*, 255-256
and CHENOWETH, L. B., Is the Human Growth Tide Turning? *10*, 548-554
and OGLE, CORDELIA, Physiologic Sterility of Adolescence, *8*, 607-615
Mine workers, cancer among, *11*, 130
MINER, JOHN R., Birth Rate and Density in a Logistic Population, *4*, 119-120
Church Membership and Commitments of Prisoners, *3*, 429-436
Church Membership and the Homicide Rate, *1*, 562-564
Correlation of Sums, *2*, 134-136
Nativity and Parentage of the Population of the United States, and the Homicide Rate, *1*, 274-278
Pierre-François Verhulst, the Discoverer of the Logistic Curve, *5*, 673-689
Restrictions on Marriage in Eighteenth Century Bavaria, *4*, 286-288
The Churches and Social Well Being, *4*, 421-428
The Variance of a Grouped Frequency Distribution When the Mean of Each Class is Taken as Centering Point, *6*, 561-563
Miner, John R., *see also* PEARL, RAYMOND
Mineral wealth and birthplace of notables, *1*, 548
Minogue, Blanche M., *see* DAVENPORT, CHARLES B.
Mirror-imaging and dermatoglyphics, *15*, 55
in twins, *6*, 24; *12*, 21
Miscarriages in first pregnancy, *1*, 399
Miscegenation, extent of, between racial stocks, *15*, 154
MISSIROLI, A., A Central Malaria Library, *6*, 240
MITCHELL, HAROLD H., A Study of Dental Caries in Porto Rican Children, *5*, 274-287
A Study of Factors Associated with the Growth and Nutrition of Porto Rican Children, *4*, 469-508
MIZUSHIMA, HARUO, Correlation between Growth Rates of Population and Vital Factors by Geographical Districts, *3*, 180-202
Mohave Indians, homosexuality among, *9*, 498
mortality among, *3*, 397
Moment equations, for slope of straight line, *12*, 271
of inertia, of human body, *10*, 363
* product-sums of curtate frequency distributions, *9*, 410

Mongolian craniology, *1*, 421
Mongolized non-Europeans, blood group B among, *14*, 433
Mongoloid imbeciles, birth order of, *8*, 256
dermatoglyphics of, *20*, 146
Monkeys, relative growth of limb segments and tail, *19*, 53
Monozygosity, diagnosis of, in human births, *3*, 114; *5*, 29; *10*, 12; *15*, 221
Monozygous twins, dental malocclusion in a pair, *20*, 36
Monster, double, *situs inversus viscerum* in, *12*, 31
MONTAGU, M. F. ASHLEY, The Significance of the Variability of the Upper Lateral Incisor Teeth in Man, *12*, 323-358
Montagu, M. F. Ashley, *see also* ASHLEY-MONTAGU, M. F.
Monthly variations in sex ratio, *10*, 57
MOORE, T. V., and HSÜ, E. H., Factorial Analysis of Anthropological Measurements in Psychotic Patients, *18*, 133-157
Moorfields crania, *6*, 316
Morbidity, following abortions, *13*, 174
infant, sex factor in, *1*, 91
racial differences in, *3*, 397
MORGAN, BANNER BILL, The Seasonal Occurence of Tularemia in the North Central States, *13*, 334-349
Moro reflex, quantitative study of the, *9*, 542, 549
Morons, female, physique of, *13*, 459
Morphological evidence of man's rationality, *2*, 459
traits, difference between morons and normal individuals, *13*, 464, 467
in gorilla, comparison of, *15*, 334
types, ancient Greek, *18*, 1
*Morphology of the palmar hypothenar dermatoglyphics in man, *7*, 1
*Mortality among patients with paranoia or paranoid conditions, *8*, 601
and chronic sickness, *18*, 33
and church membership, *4*, 424
associated with organ systems, secular changes in, *3*, 494
at advanced ages, *1*, 558
Australian aborigines, *12*, 501
brewery employees, *1*, 136
cardiovascular disease, in parents and offspring, *13*, 350
childhood, in medieval England, *9*, 534
comparative, and life table death rates, *18*, 127
experience of British Annuitants, 1900-1920, *11*, 19
familial, *13*, 190
* in brother-sister and husband-wife pairings, *13*, 189
* in husband and wives, *12*, 508; *13*, 507
* in New York City a century and a half ago, *6*, 87
in New York State, by country of origin and cause, *17*, 340
infant, *see* Infant mortality
Negro, *1*, 229; *3*, 71, 203, 397
neonatal, *11*, 344
* of the population of Greece, a comparative study of, *8*, 553

(Mortality)
of twins, *12*, 247
prenatal, and parental age, *12*, 430
qualitative changes in, *6*, 63
racial differences in, *3*, 397
rates, late fetal, *11*, 354
seasonal variation in sex ratio, *1*, 104
Seminole Indians, *7*, 341
* social class, *17*, 1
trends, among Negroes, *17*, 216
in the United States, 1830-1930, *14*, 358
use of life table death rates for comparative, *18*, 127, 238
Movements of teeth, in children, *8*, 161
MUHSAM, H. V., Correlation in Growth, *19*, 260-269
Multiple births, *see* Twins, Triplets
* dichotomy, factorial analysis of, *20*, 195
* human births, diagnosis of zygosity in, *15*, 221
MURPHY, MILES, The Birth Order of Mongol and Other Feeble-minded Children, *8*, 256-266
Murray, Henry A., Jr., *see* BARRY, HERBERT, JR.
Muscle exertions of arms, *11*, 533
Musculature, manual, phylogeny of, *13*, 215
MYERS, ROBERT J., A Note on the Variance of Sex Ratios, *15*, 267-270
Further Note on the Death Rate among Old-Age Assistance Recipients in Rhode Island, *15*, 175-178
MYSLIVEC, VÁCLAV, Contribution to the Analysis of the Exceptions to Bernstein's Theory, *17*, 13-45

*Names of the falling sickness, *2*, 109
Nasal breadth and temperature, *2*, 246
diseases in Negroes, *3*, 215
index of female morons, *13*, 462
Nationality and mortality, *17*, 340
Native-born women, fertility of, *8*, 519
*Nativity and marriage in New York State, *3*, 376
* and parentage of the population of the United States, and the homicide rate, *1*, 274
of family head, and permanence of residence, *19*, 109
Natural increase, immigration, and population growth, *8*, 25
of population, true rate of, *3*, 182, 476
* selection in the processes of population growth, *1*, 503
*Nature and nurture in relation to twins and brothers in *Who's Who*, *5*, 371
Neck girth of different disease groups, *6*, 693; *8*, 69
*Negative association between intelligence and fertility, *13*, 410
binomial distribution, of abnormal blood pressure examinations, *19*, 49
Negritoes, theories of origin of, *2*, 42
Negro(es), abortion in, *3*, 233
adaptation to exercise, sharecroppers, *13*, 139

adolescent weight and stature, *10*, 124
body build of Tuskegee, *15*, 128
calcification of costal cartilage, *6*, 543
color blindness among, *8*, 451
cranial capacity, *14*, 423
developmental differences between, and whites, *4*, 9
differential mortality in the American, *3*, 71, 203
environmental trends, *17*, 207
fertility, *8*, 520
hiatus canalis sacralis in, *17*, 368
* infant, the, *4*, 1
marriages in New York State, *3*, 393
mental capacity of, *1*, 70
disease among, *7*, 471; *11*, 513
mortality, biological factors in, *1*, 229
percentage in population of Washington County, Maryland, *12*, 63
performance on form discrimination test, *13*, 239
permanence of residence, Baltimore, *19*, 91
personality disorders among, in Baltimore, *11*, 114, 485
in Georgia, *11*, 513
physical features, *1*, 57
pigmentation, age changes of, *1*, 334
inheritance of, *1*, 321
population in United States, *1*, 263
respiratory infection in, *14*, 105
social definition of, *1*, 255
stillbirth rates, *17*, 95
West African, cephalic measurements, *9*, 484
NELSON, SEVERINA E., Personal Contact as a Factor in the Transmission of Stuttering, *11*, 393-401
Neonatal mortality rates, *11*, 344
Nephritis and weight, *2*, 170
in Negroes, *3*, 222
mortality, and nationality, *17*, 349
*"Nerves" and the public health, *4*, 155
Nerves, antebrachio-manual extensors, phylogeny of, *13*, 217
Nervous diseases, somatology of, in Army officers, *5*, 89
system, diseases of, in Negroes, *3*, 228
in old age, *4*, 93, 107
Neumann, C., *see* BURCH, G. E.
NEUMANN, GEORG, American Indian Crania with Low Vaults, *14*, 178-191
Neuro-central patterns of motivation, *10*, 94
*Neuropathology and the internal environment, *7*, 74
Neuroticism and age at marriage, *16*, 21
Nevada Indians, racial fusion among, *15*, 153
New-born, anthropometry of the, *6*, 612
New Ireland, vital statistics of, *3*, 551

New World populations, comparative study of, *12*, 359
New York City, mortality in, 1804-08, *6*, 87
New York State, homicide in, *7*, 47
marriages in, *3*, 376
mental disease among Negroes in, *7*, 471
mortality in, by nationality, *17*, 340
NEWMAN, H. H., The Question of Mirror Imaging in Human One-Egg Twins, *12* 21-34
NEWMAN, MARSHALL T., A Comparative Study of Finger Prints and Toe Prints, *8*, 531-552
Nipple height, maximum, among boys, *13*, 543
Nisei women, anthropometry of, *14*, 280
Nitrogen excretion and body measurements, *4*, 351
and ingestion, *7*, 419; *9*, 405; *10*, 400
*Nomogram for computation of probable errors of differences, *11*, 543
* for rapid computation of the blood-grouping genes in populations, *6*, 558
Non-Jewish whites, personality disorders among, *11*, 486
Non-selective deaths, effect on sex ratio, *3*, 566
Normal ages, Lexis' conception of, *11*, 9
variability in height and weight, *5*, 463, 617
weight, prediction of, *11*, 473
Norms, anthropometric, for working women, *6*, 472
for ossification of hand and wrist, *6*, 467
Nose height, of schizophrenics, *11*, 360
*Notables, American, ecology of, *1*, 544
*Note on a new sliding calipers for cranial measurements, including internal diameters, *9*, 417
* on the death rate among persor ceiving old-age assistance in Rhode Island, *14*, 388
* on the estimation of dysplasia in human physiques: Sheldon's method and the analysis of variance, *14*, 110
* on the problem of mirror-imaging in human twins, *6*, 24
* on the statistical significance of the difference of two series of comparable means, *6*, 402
* on the variance of sex ratios, *15*, 267
*Notes on a family of breeding gibbons, *5*, 288
* on Mongolian craniology, *1*, 421
NOTESTEIN, FRANK W., and KISER, CLYDE V., Fertility of the Social Classes in the Native White Population of Columbus and Syracuse, *6*, 595-611
Nutrient foramen of radius, *11*, 369
Nutrition of Puerto Rican children, *4*, 469

Oakland, California, differential fertility in, *7*, 542
Obesity, an extraordinary case of extreme, *14*, 166
effect on specific gravity, *5*, 668
*Oservational study of two captive mountain gorillas (*Gorilla beringei*), *9*, 175
*Observations on the growth, classification and evolutionary specialization of

gibbons and siamangs, *5*, 212, 385
* on the size and position of the nutrient foramen in the radius, *11*, 369
Obsessions, and feelings of unreality, *1*, 514
Occupational classification and code, *5*, 491
disabilities, personality factor in, *4*, 165
Occupation and age at marriage, Washington County, Md., *12*, 69
of father, and birth rate, *2*, 511; *12*, 193
preferred by twins, *6*, 346
Offspring, number of, in relation to education of parents, *12*, 117
Ogle, Cordelia, *see* MILLS, CLARENCE A.
Old-age assistance recipients in Rhode Island, death rate among, *14*, 388; *15*, 175
blood pressure in, *2*, 264
*Operational methods in problems of fissiparous propagation, *8*, 126
Oral temperature correlated with rectal temperature, *7*, 24
Orang-utan, relative growth in, *11*, 385
Organ systems, relative importance of, in mortality, *3*, 501
Organism-as-a-whole, concept of, and psychiatry, *8*, 489
Organization for Research in Population, *6*, 223
measure of, *19*, 207
Organological classification of causes of death, *1*, 237
ORTMANN, OTTO, Theories of Synesthesia in the Light of a Case of Color-hearing, *5*, 155-211
OSBORN, HENRY FAIRFIELD, Is the Ape-Man a Myth? *1*, 4-9
*Ossicula wormiana, *18*, 61
Ossification of hand and wrist, *6*, 454
* time of the os pisiforme (Studies in the physical development, V), *17*, 143
*Outcome of 1556 conceptions: a medical and sociological study, *13*, 159
Ovary, weight of, *6*, 514; *7*, 578
Overweight, and causes of death, *2*, 159

Pai, T., *see* STEVENSON, P. H.
Palatability differences, and appetite levels, *15*, 202
Palate length-breadth index, in gorilla, *15*, 308
in langurs and macaques, *14*, 461
*Paleolithic, European races of the upper, *7*, 196
*Palestine, population growth in, *9*, 347
Palm prints, *see* Dermatoglyphics
*Palmar dermatoglyphics of Mongoloid imbeciles, a study of, *20*, 146
PALMER, CARROLL E., Note on the Statistical Significance of the Difference of Two Series of Comparable Means, *6*, 402-405
The Relationship of Erect Body Length to Supine Body Length, *4*, 262-271
and REED, LOWELL J., Anthropometric Studies of Individual Growth. I. Age, Height, and Rate of Growth in Height, Elementary School Children, *7*, 319-334
Palmer, Carroll E., *see also* CIOCCO, ANTONIO
Pan-American League against Cancer, *13*, 252
Pancreas, diseases of, and sex, *2*, 73

Parabolas, mean of, *3*, 43
Paradox of the greatest age, Gumbel's *11*, 2
Paralysis, and weight, *2*, 171
Paranoia, mortality among patients, *8*, 601
Paranoid type, body build of, *11*, 364
Parathyroids, weight of, *6*, 502; *7*, 555; *9*, 245
*Parental age and prenatal mortality, *12*, 430
-filial relationship, and dermatoglyphic resemblance, *10*, 23
Parity-age contours, *9*, 92
*Parity order of birth, age of mother, and socio-economic status, *13*, 64
Parkhurst, Elizabeth, *see* DePorte, J. V.
Parr, Thomas, autopsy of, *3*, 138
Parsi demography, *20*, 49
Pasteurization of milk, and infant mortality, *9*, 156
Paternity, exclusion of, *5*, 580; *7*, 236
in Sendai, Japan, *2*, 508
Pathological conditions of gorilla skeleton, *16*, 63
Patron saints of epilepsy, *2*, 121
Patterns of activity in the Moro reflex, *9*, 545
of head growth, *11*, 202
of reproductivity in man and other mammals, compared, *9*, 79, 238
Peacock, Thomas Love, on craniology, *1*, 139
Pearl, Raymond, portrait of, preceding *12*, 457
Pearl, Raymond, A Classification and Code of Occupations, *5*, 491-505
A Comparative Examination of Certain Aspects of the Populations of the New World, *12*, 359-403
An Eighteenth Century French Evolutionist, *2*, 559-566
An Explanatory Note regarding a More Detailed Classification of Recent Literature Useful in the Study of Human Biology, *4*, 131-133
Biological Factors in Negro Mortality, *1*, 229-249
Changes in the Incidence of Mortality at Advanced Ages, *1*, 558-561
Cobbes Questions and Answers, *2*, 137-144
Constitutional Somatology, *1*, 283-284
Contraception and Fertility in 2000 Women, *4*, 363-407
Contraception and Fertility in 4945 Married Women, *6*, 355-401
Dr. John Cross on Physiognomy and Constitution, *1*, 426-430
Foreword, *1*, 1-3
Harvey's Post-Mortem Examination of Thomas Parr, *3*, 138-142
John Hunter on Appetites and Aging, *1*, 565-571
Longevity: a Pedigree, *3*, 133-137
Mr. Cranium's Lecture, *1*, 139-142
Mortality of Brewery Employees, *1*, 136-138
On the Alleged Lethal Effects of Sexual Continence in the Holy Land, *3*, 577-585
On the Moment Product Sums of Curtate Frequency Distributions, *9*, 410-416
On the Weight of the Parathyroids, *9*, 245-250
Potential and Actual Reproductive Performance in Man, *8*, 508-530

Pregnancy Rates and Coitus Rates, *12*, 545-558
Records of Total Reproduction, *11*, 144-146
Some Data on Fertility and Economic Status, *4*, 525-553
Studies on Human Longevity. IV. The Inheritance of Longevity, *3*, 245-269
The History of a Gesture, *2*, 566-568
Tobias Venner and his *Via Recta*, *4*, 558-583
Variation in Parity of Women Bearing Children in the U. S. Birth Registration Area in 1930, *9*, 65-98, 212-244
et al., Studies on Constitution. I. Methods, *1*, 10-56
and CIOCCO, ANTONIO, Studies on Constitution. II. Somatological Differences Associated with Diseases of the Heart in White Males, *6*, 650-713
GOOCH, MARJORIE, and FREEMAN, WALTER, A Biometric Study of the Endocrine Organs in Relation to Mental Disease, *7*, 350-391, 555-607
GOOCH, MARJORIE, MINER, JOHN R., and FREEMAN, WALTER, Studies on Constitution. IV. Endocrine Organ Weights and Somatological Habitus Types, *8*, 92-125
and GOULD, SOPHIA A., World Population Growth, *8*, 399-419
and MINER, JOHN R., On the Comparison of Groups in Respect of a Number of Measured Characters, *7*, 95-107
and PEARL, RUTH DEWITT, Studies on Human Longevity. VI. The Distribution and Correlation of Variation in the Total Immediate Ancestral Longevity of Nonagenarians and Centenarians, in Relation to the Inheritance Factor in Duration of Life, *6*, 98-222
and RAENKHAM, T., Studies on Human Longevity, V. Constitutional Factors in Mortality at Advanced Ages, *4*, 80-118
Peck order, measure of, *19*, 202
Pecos crania, *6*, 309
Pedigree of higher primates, *2*, 402
PELLER, SIGISMUND, Further Studies on the Role of Irritation in Cancer, *9*, 57-64
Lung Cancer among Mine Workers in Joachimsthal, *11*, 130-143
Pelvis, breadth of, in schizophrenics, *11*, 365
measurements, of gorilla, *16*, 35
of gibbons, *5*, 245
of langurs and macaques, *14*, 452
of primates, *2*, 346
white and Negro, *1*, 61
width, growth of, *7*, 164
*Penetrance, estimated by the frequency of unilateral occurrences and by discordance in monozygotic twins, *19*, 217
Pennell, Elliott H., *see* COLLINS, SELWYN D.
Percentage index of body build, definition and use, *11*, 473
*Perception and language, *1*, 555
*Permanence of residence with respect to various family characteristics (Family studies in the Eastern Health District, IV), *19*, 91
of households in Eastern Health District of Baltimore, *15*, 120
*Personal appearance and neuroticism, as related to age at marriage, *16*, 15
* as related to scholastic records and marriage selection in college women, *10*, 65

(Personal)
* contact as a factor in the transmission of stuttering, *11*, 393
Personality, alterations in disease, *1*, 458
analysis of, and medicine, *1*, 131
* and marriage, biological factors in, *7*, 430
diagnosis of, *4*, 168
* disorder in the Eastern Health District in 1933 (Statistical contributions from the Mental Hygiene Study of the Eastern Health District of Baltimore, III and IV), *11*, 112, 485
factor in occupational disabilities, *4*, 165
* studies in, A. Hypnotizability, *3*, 1
Petit-mal attacks, disturbance of vision and consciousness in, *2*, 547
PHAIR, JOHN J., SCHOENBACH, EMANUEL B., and MERRELL, MARGARET, Chemoprophylaxis in the Prevention of Disease with Especial Reference to Meningococcal Infection. I. A Comparative Study of the Absorption, Persistence, and Excretion of Four Sulfonamide Compounds, *18*, 171-203
Phenyl-thio-carbamide, taste reactions to, in Baghdad population, *13*, 400
PHILLIPS, GILBERT, The Blood Groups of the Maori, *3*, 282-287
Phillips, W. S., *see* GREENE, J. E.
Phosphorus, balance in pregnancy, *9*, 34
Photisms, and theories of synesthesia, *5*, 155
Photographic records in constitution study, *1*, 18, 40, 44
Photographs of patients with sickle cell anemia, *16*, 102
Phylogenetic principles and human evolution, *1*, 7, 160
*Phylogeny of the human forearm extensors, *13*, 23, 203
* of the primates, *1*, 214
of the thoracic index of man, *6*, 14
*Physical abilities, sex differences in, *19*, 12
age, computation of, *2*, 491
development, and season of conception, *13*, 378
* in relation to menarcheal age in university women, *8*, 198
* studies in, *17*, 143, 147, 207, 229
fitness, criterion of, *15*, 299
quotient and intelligence quotient, *2*, 473
constancy of, *2*, 502
status of Tennessee mountain children, *5*, 706
types, a statistical fallacy involving, *13*, 541
indices of, *3*, 420
* of West African Negroes, *9*, 483
Physiognomy, Dr. John Cross on, *1*, 426
Physiologic events, chronology of, *17*, 147
* sterility of adolescence, *8*, 607
Physiological differences between Negroes and whites, *4*, 6
responses, seasonal, stability of, *14*, 74
variables, intercorrelations between, *14*, 75
*Physique in hereditary mental defect: an anthropometric study of 97 "Old American" female morons, *13*, 459

* of women students at Newcomb College of Tulane University. IV. Stature, weight and body build by national extraction, *5*, 122; V. Increase in stature at college, based on repeated measurements of the same individual, *11*, 220
and menarcheal age, *9*, 1
and psychotic type, *18*, 133
PIERACCINI, A., On the Priority of the Biological Hereditary Law of "Anticipation," *4*, 554-557
Pigmentation, age changes in Negroes, *1*, 334
inheritance in Negro, *1*, 321
measurement technique, *1*, 322
of West African Negroes, *9*, 489
Pignet's index, *3*, 421
Pineal body, weight of, *6*, 492
weight, *7*, 581
Pisiforme, appearance of, *17*, 143
Pithecanthropus, *1*, 5, 155, 158
Pitney, E. H., *see* ABERLE, S. D.
PITT-RIVERS, GEORGE, Anthropological Approach to Ethnogenics: A New Perspective, *4*, 239-251
Pituitary body, weight of, *6*, 494
fossa, area in children, *20*, 122
growth, *20*, 1
weight, *7*, 565
*Placentation studies: correlations between size of sac, area of placenta, weight of placenta and weight of baby, *11*, 248
Plantar patterns of chimpanzee, *10*, 495
of Indians and Spanish-Americans, *13*, 185
Pneumonia, and weight, *2*, 178
in Negroes, *3*, 203
infants, *4*, 25
mortality and country of origin, *17*, 354
*Point of population saturation: its transgression in Mauritius, *1*, 528
Poisson frequency distribution, *19*, 154
*Political parsons, *1*, 284
POLL, HEINRICH, Two Unlike Expressions of Symmetry of Finger-Tip Patterns, *10*, 77-92
*Polyisomerism and anthropogeny, *6*, 632
Polynesians, racial composition of, *2*, 47
POMERAT, CHARLES M., Homogamy and Infertility, *8*, 19-24
POMERAT, GERARD ROLAND, Fertility in Relation to Age at Time of Marriage, *8* 420-432
Ponderal index, definition and age variation of, *11*, 482
Pontico, Mrs. Ruth C., an extraordinary case of obesity, *14*, 167
Population, aboriginal survivorship, *19*, 83
* an Australian aboriginal, *12*, 481
and food supply, *5*, 42
Association of America, *6*, 225

(Population)
by social class, England and Wales, 1931, *17*, 3
changes in, during the eighteenth century in Mexico, *14*, 513
characteristics in Washington County, and Hagerstown, Maryland, *12*, 63
of San Juan pueblo Indians, *12*, 152
China, *19*, 1
control, Puerto Rico, *20*, 21
density of New World, *12*, 364
and miscegenation, *15*, 154
French, eighteenth century, Messance on, *12*, 77
* growth due to immigration and natural increase, *8*, 25
* equilibria, and extinction under specified breeding conditions: a development and extension of the theory of the logistic curve, *10*, 1
* in Palestine, *9*, 347
* in Puerto Rico and its relation to time changes in vital statistics, *17*, 267
Malthus on checks to, *10*, 214
Messance on checks to, *12*, 87
natural selection in, *1*, 503
of nations, *18*, 204
of Seminole Indians, *7*, 335
rate and age distribution, *3*, 194
rates, in New World, *12*, 369
Index, *9*, 251
Iowa female, *7*, 514
isolated refugee, of the Caucasus, and blood group B, *14*, 431
logistic, age distribution, *3*, 470
non-European of U.S.S.R., and blood group B, *14*, 433
* of Mexico in 1793, *14*, 499
Parsi in India, *20*, 49
pre-colonial Mexico, *18*, 81
problem in Japan, *4*, 408
research, organization for, *6*, 223
saturation, in Mauritius, *1*, 528
Spanish, in San Juan pueblo area, *12*, 157
stationary, a study of, *7*, 514
of early man, *5*, 36
* statistics in the kingdom of Dahomey, *4*, 252
structure of growing, *3*, 459
trends of San Juan pueblo, *12*, 148
true rate of natural increase of, *3*, 182, 476
world, *8*, 399
Post-natal growth, in first ten days, *11*, 24
Postural reactions in cerebral disease, *1*, 410
*Potential and actual reproductive performance in man, *8*, 508
Potgieter, Martha, *see* SEEGERS, WALTER H.
see also SONTAG, L. W.
Potter, Evelyn A., *see* LOMBARD, HERBERT L.

POWDERMAKER, HORTENSE, Vital Statistics of New Ireland (Bismarck Archipelago) as Revealed in Genealogies, *3*, 351-375
Prediction of normal weight, *11*, 473
* of stature of North European males throughout the elementary school years, *8*, 279
Preference, food, and appetite levels, *15*, 213
Pregnancy, calcium and phosphorus balances during, *9*, 29
changes in systolic blood pressure, heart rate and temperature, *8*, 232
excretion and metabolism during, *10*, 528
nitrogen excretion during, *9*, 404
order of, and duration of marriage, *14*, 14
* rates and coitus rates, *12*, 545
and contraception, *4*, 401, 532; *6*, 386; *10*, 382; *14*, 6
*Preliminary report on the measurement of ossification of the hand and wrist, *6*, 454
Pre-marital relationship, disturbing conditions in, *11*, 236
Premature birth and parental age, *11*, 350
*Prenatal development of the hair-tracts in primates, *12*, 203
environment of Negroes, *4*, 10
mortality, parental age and, *12*, 430
Presbyopia, development of, *7*, 260
PRESTON, MALCOLM, Mental Status and Season of Conception, *14*, 251-255
*Prevalence of pulmonary tuberculosis among selectees, *16*, 181
Preventive medicine, industrial, *19*, 27
Primate(s), density of hair in, *3*, 303
hair tracts, *12*, 203
phylogeny, *1*, 214; *2*, 99, 402
skeleton of, *2*, 303
skulls, types of, *2*, 104
Primitive behavior, reversion to, from organic brain disease, *1*, 406
*Primogeniture and developmental anomalies, *1*, 382
*Priority of the biological hereditary law of "anticipation," *4*, 554
*Probabilities of death in closed population groups, illustrated by probabilities of death of white fathers after birth of children, *18*, 158
*"Probability and Listerism," *20*, 161
curves, arithmetic and logarithmic, *13*, 7
* of death and expectation of life, *18*, 238
of dying from various causes, *6*, 68
theory in contest, *19*, 190
Probable errors of differences, nomogram for, *11*, 543
Pro-dawn man, is he a myth? *1*, 153
*Prolificacy distribution of white wives according to fertility tables for the Registration Area, *12*, 35
Propagation, fisssiparous, mathematics of, *8*, 126
*Proportions, variability and asymmetries of the long bones of the limbs and the clavicles in man and apes, *9*, 281
*Proposal for the compulsory anthropological study of emigrants, *2*, 557
Prostate, weight of, *6*, 514

Protein intake, and nitrogen excretion, *7*, 419; *9*, 408
Psychiatric status and somatic habitus, *14*, 203
Psychiatry, biometrical studies in, *4*, 208
Psychobiological factors in chimpanzee mating, *11*, 94
* study of a set of identical girl triplets, *5*, 1
*Psychogenetic relationship, the, *4*, 280
*Psychological and physical concomitants of adjustment in marriage, *12*, 559
differentiation, among Chinese, *9*, 457
factors and vital indices, *5*, 94
tests of identical triplets, *5*, 26
Psychotic type, relationship to anthropological measurements, *18*, 133
Pubertal girls, menstrual history of, *6*, 431
Pubic hair, development of, *2*, 486
distribution, sex differences in, *17*, 137
Public health and "nerves," *4*, 155
Puerperal deaths in Negroes, *3*, 231
Puerto Rico, birth control in, *20*, *21*
children, dental caries in, *5*, 274
growth and nutrition of, *4*, 469
life tables, *17*, 282
population growth, *17*, 267
Pulmonary tuberculosis, somatology of, in Army officers, *5*, 62
prevalence among white and Negro selectees, *16*, 181
Pulse and blood pressure, in enlisted men, *5*, 568
blood pressure in Japanese women, *14*, 328
rate of army officers, *4*, 516
in the long-lived and short-lived, *15*, 168
Pyknic type, in schizophrenic women, *14*, 206
PYLE, S. IDELL, Bone Weight in the Human Carpus, *7*, 108-118
Interrelations of Hemoglobin, Basal Metabolism, and Creatine, Creatinine and Magnesium Excretion during Human Pregnancy, *10*, 528-536
Observations on the Size and Position of the Nutrient Foramen in the Radius, *11*, 369-378
Pyle, S. Idell, *see also* HUFF, C. ELTON
Pylorus, congenital hypertrophy of, and primogeniture, *1*, 393

*Quantitative analysis of growth in certain human skull dimensions, *14*, 143
*Quantitative aspects of organization, *19*, 163
* behavior data and the longitudinal method. The Moro reflex, *9*, 542
* study in human teratology, *2*, 1
* theory of organic growth (Inquiries on growth laws, II), *10*, 182
*Quantity of creatine and creatinine excreted in normal human pregnancy, *9*, 404
*Quartile abscissa of the normal curve to 15 places of decimals, *11*, 285
*Question of mirror-imaging in human one-egg twins, *12*, 21

Rabbits, growth in weight of, *3*, 53
Race(s) and blood groups, *12*, 475

and dimensions of new-born, *6*, 619
classification of, *8*, 365; *13*, 390
criteria of, *2*, 38
definition of, *14*, 373
do they differ in mental capacity? *1*, 70
Mediterranean, and blood group B, *14*, 430
Nordic, and blood group B, *14*, 430
of Europe, *2*, 53
* of the world: a discussion of recent classifications, *13*, 390
pre-Mendelian conception of, *14*, 373
* type, and ethnic group in ancient Greece, *18*, 1
*Racial and regional differences in standard therapy rates among white and Negro mental patients, *11*, 513
assimilation of the American Indian, *15*, 33
composition, estimates of, New World populations, *12*, 396
of Mexico in 1793, *14*, 500
decline, factors in, *4*, 245
differences in extent of hiatus canalis sacralis, *17*, 368
in menarcheal age, *14*, 335
* in mortality and morbidity, *3*, 397
* in resistance to respiratory infection, *14*, 104
differentiation, alleged functional factors in, *2*, 237
factor in the variability of lateral incisors, *12*, 336
* fusion among the California and Nevada Indians, *15*, 153
* migration-zones and their significance, *2*, 34
origin, of mothers, and infant mortality, *12*, 234
susceptibility to disease, *1*, 125
Racing, horse, as example of organization, *19*, 181
Radius of primates, *2*, 360
Raenkham, T., *see* PEARL, RAYMOND
RAIFORD, T. E., The Measurement of Variations in Asymmetrical Data with Applications to Hemoglobin Statistics of Infants, *10*, 136-144
The Relationship between Height and Weight of Male and Female Infants between the Ages of Two Weeks and One Year, *10*, 409-416
RANDALL, FRANCIS E., The Skeletal and Dental Development and Variability of the Gorilla, *15*, 236-254, 307-337; *16*, 23-76
Rate constants, absorption and elimination of sulfonamides, *18*, 184
* of growth of the Chinese population, notes on, *19*, 1
Ratio of Indians to whites in California, *15*, 36
RAWLES, THOMAS H., Operational Methods in Problems of Fissiparous Propagation, *8*, 126-132
Reactions to marital conflict, *10*, 566
*Recent changes in the secondary sex ratio of the upper social strata, *20*, 182
RECENT LITERATURE, *1*, 144-152, 286-304, 431-444, 572-588; *2*, 145-158, 292-302, 441-456, 569-581; *3*, 143-156, 289-301, 442-458, 586-600; *4*, 131-153, 289-305, 435-451, 584-602; *5*, 135-154, 295-312, 520-540, 722-740; *6*, 241-270, 408-430, 564-593, 722-737; *7*, 121-150, 272-301, 443-469, 616-639; *8*, 134-160, 284-308, 459-487, 617-

(RECENT LITERATURE)
642; *9*, 108-132, 253-280, 419-449, 572-595; *10*, 151-180, 300-326, 425-454, 580-606; *11*, 148-172, 290-318, 410-446, 550-579; *12*, 121-140, 287-312, 432-455, 567-592; *13*, 114-137, 257-282, 413-438, 547-569; *14*, 115-140, 257-278, 389-412, 534-555; *15*, 80-100, 179-197, 271-293, 338-358; *16*, 77-98, 132-152, 190-205, 298-314; *17*, 46-63, 151-165, 243-266, 382-397
*Records of total reproduction, *11*, 144
Rectal and oral temperature, correlation of, *7*, 24
Recumbent lengths of children, daily decreases in, *15*, 67
REED, LOWELL J., and LOVE, ALBERT G., Biometric Studies on U. S. Army Officers: Somatological Norms, Correlations, and Changes with Age, *4*, 509-524; Somatological Norms in Disease, *5*, 61-93
Reed, Lowell J., *see also* PALMER, CARROLL E.
Regression equations, logarithmic, *14*, 95
lines, methods of fitting, *19*, 231
of pigmentation of offspring on parents, *1*, 342
*Relation between inbreeding and intelligence, *20*, 171
* of country of origin to mortality for various causes in New York State, *17*, 340
* of season of birth to certain attributes of students, *13*, 533
* of the mode of inheritance to the severity of an inherited disease, *4*, 69
*Relationship between bodily and gastric activity of newborn infants, I. Correlation and influence of time since feeding, *8*, 368; II. Simultaneous variations in the bodily and gastric activity of newborn infants under long-continued light stimulation, *8*, 381
* between epilepsy and maternal age, *18*, 49
* between height and weight of male and female infants between the ages of two weeks and one year, *10*, 409
* between menarcheal age and certain measurements of physique in girls at ages 9 to 16 years, *9*, 1
* between the length and weight of the newborn infant and the height and weight of the mother, *8*, 565
* of erect body length to supine body length, *4*, 262
* of individual to average growth, *3*, 37
*Relative growth of the limb bones in the anthropoid apes, *11*, 379
* of the limb segments and tail in *Ateles geoffroyi* and *Cebus capucinus*, *19*, 53
* of the limb segments and tail in macaques, *13*, 283
value of sources of cancer information, *16*, 121; *20*, 201
Relaxability, of skeletal musculature, *14*, 79
*Reliability of dermatoglyphics in twin diagnosis, *10*, 12
Relief status, and permanence of residence, *19*, 117
Religio Medici, *1*, 283
*Reply to the criticism of Malcolm G. Preston, *14*, 255
Reproduction rates, China, *19*, 7
generation, *19*, 133
net, *17*, 191
Reproductive performance in man, *8*, 508

wastage rates and family income, *4*, 546
Reproductivity, adequate, *9*, 88, 238
*Resemblances in the auditory acuity of siblings (Studies on pupils of the Pennsylvania School for the Deaf, 2), *11*, 259
Respiratory admission rates, U. S. Army, by month, correlated, *20*, 90
diseases, weather, and depressions, *10*, 390
infections in Negroes, *3*, 203
racial differences in resistance to, *14*, 104
system in old age, *4*, 89, 101
*Restrictions on marriage in eighteenth century Bavaria, *4*, 286
*Reversion to primitive behavior resulting from organic disease of the brain, *1*, 406
Reynolds, H. H., *see* LASKER, GABRIEL W.
Rh agglutinogen frequencies among Gentile and Jewish students, *16*, 175
* blood factor among twins, *17*, 195
Rhinitis in Negro infants, *4*, 25
Rhythm discrimination, in whites and Negroes, *1*, 78
RICHARDS, T. W., The Relationship between Bodily and Gastric Activity of Newborn Infants: I. Correlation and Influence of Time since Feeding, *8*, 368-380; II. Simultaneous Variations in the Bodily and Gastric Activity of Newborn Infants under Long-continued Light Stimulation, *8*, 381-387
LE RICHE, HARDING, Comparative Physical Growth in Eurafrican and European Juvenile Adults, *11*, 319-341
Rickets in Negroes, *3*, 221; *4*, 22
RIDER, ROWLAND V., and BADGER, GEORGE F., Family Studies in the Eastern Health District. III. A Consideration of Issues Involved in Determining Migration Rates for Families, *15*, 101-126
RIFE, DAVID C., A Common Misconception concerning the Behavior of Linked Factors, *11*, 546-548
Handedness and Dermatoglyphics in Twins, *15*, 46-54
and CUMMINS, HAROLD, Dermatoglyphics and Mirror-Imaging, *15*, 55-64
and SCHONFELD, MURRY D., A Comparison of the Frequencies of Certain Genetic Traits among Gentile and Jewish Students, *16*, 172-180
and SNYDER, LAURENCE H., Studies in Human Inheritance. VI. A Genetic Refutation of the Principles of "Behavioristic" Psychology, *3*, 547-559; XVI. The Distribution of Sex Ratios within Families in an Ohio City, *9*, 99-103
Rife, David C., *see also* CROMWELL, HOWARD
RITTER, WILLIAM E., Is Man a Rational Animal? *2*, 457-472
ROBERTS, J. A. FRASER, The Negative Association between Intelligence and Fertility, *13*, 410-412
ROBINOW, MEINHARD, The Statistical Diagnosis of Zygosity in Multiple Human Births, *15*, 221-235
ROBINSON, S., DILL, D. B., HARMON, P. M., HALL, F. G., and WILSON, J. W., Adaptations to Exercise of Negro and White Sharecroppers in Comparison with Northern Whites, *13*, 139-158
ROSAHN, PAUL D., A Graphic Method for Representing the Significance of the Difference between Means, *7*, 267-271

Rotation of body, in walking, *11*, 531
Rothberg, Sidney, *see* WIENER, ALEXANDER S.
ROWE, ALLEN WINTER, Vital Function Studies. XIII. The Influence of Abnormal Sitting Height Indices on Weight and Lung Volume Predictions, *7*, 240-255
Royal pedigrees, analyzed for relationship between intelligence and inbreeding, *20*, 173
Rural children, dental caries in, *5*, 275
fertility, 1900-1910, *5*, 256
population, correlation with growth-rate of population, *3*, 192
RUSSELL, JOSIAH COX, Length of Life in England, 1250-1348, *9*, 528-541
Russia, legalization of abortion in, *6*, 280
Russian folk medicine, *3*, 576

St. Kilda, upper respiratory disease in, *3*, 437
Sample problems, important unsolved, *20*, 205
problems in measuring penetrance, *19*, 223
San Diego Zoological Gardens, *9*, 177
*San Juan pueblo, vital history of, *12*, 141
Sarcoma of digestive tract, and sex, *2*, 90
Saturation of population, in Mauritius, *1*, 528
Scammon, R. E., and L. A. Calkins, *15*, 2
Scammon, R. E., *see also* WILMER, HARRY A.
Scapula of langurs and macaques, *14*, 450
of primates, *2*, 339
Scapular angle, growth changes in, *7*, 183
Scarlet fever in Negroes, *3*, 72
Schizophrenia, body build in, *11*, 357; *14*, 21, 192
Schizophrenics, temperature of, *7*, 24
Schizothymes, Chinese, *9*, 459
Schoenbach, Emanuel B., *see* PHAIR, JOHN J.
Schonfeld, Murry D., *see* RIFE, DAVID C.
School attendance, correlation with vital indices, *5*, 94
children, New Haven, of Italian ancestry, physical fitness of, *15*, 297
subjects preferred by twins, *6*, 345
SCHREK, ROBERT, Logarithmic Correlation Coefficients and Regression Equations, *14*, 95-103
and LIPSON, HENRY I., Logarithmic Frequency Distributions, *13*, 1-22
SCHULTZ, ADOLPH H., Inherited Reductions in the Dentition of Man, *6*, 627-631
Observations on the Growth, Classification and Evolutionary Specialization of Gibbons and Siamangs, *5*, 212-255, 385-428
Proportions, Variability and Asymmetries of the Long Bones of the Limbs and the Clavicles in Man and Apes, *9*, 281-328
Raymond Pearl, Founder and Editor of Human Biology, preceding *12*, 457
The Density of Hair in Primates, *3*, 303-321
The Hereditary Tendency to Eliminate the Upper Lateral Incisors, *4*, 34-40
The Skeleton of the Trunk and Limbs of Higher Primates, *2*, 303-438
Schultz, Adolph H., *see also* LUMER, HYMAN

Schultz scale for estimating growth stages of gorilla, *15*, 239
Seashore test of time and rhythm discrimination, *1*, 75
*Season of birth related to certain attributes of students, *13*, 533
* of conception and mental and physical development, *13*, 378
and mental status, *14*, 251, 255
*Seasonal cycle in human growth, evidence of a, *2*, 523
* occurrence of tularemia in the North Central States, *13*, 334
variations in dimensions of newborn, *6*, 618
Secondary effects of injury, *1*, 120
*Secular changes in mortality rates connected with certain organ systems, *3*, 494
SEEGERS, WALTER H., and POTGIETER, MARTHA, The Quantity of Creatine and Creatinine Excreted in Normal Human Pregnancy, *9*, 404-409
Segregation, effect on racial mixture, *15*, 155
SEIBERT, HENRI C., Observations on the Somatic Constitution of Mothers with and without Infant Mortality among Their Progeny, *12*, 232-246
SEKAR, C. CHANDRA, Some Aspects of Parsi Demography, *20*, 47-89
Selection in population growth, *1*, 503
of children, in study of physical fitness, *15*, 296
Self-destructive activities of animals, *2*, 465
Self-ratings of physical appearance, reliability of, *16*, 18
SELTZER, CARL C., The Anthropometry of the Western and Copper Eskimos, Based on Data of Vilhjalmur Stefansson, *5*, 313-370
Seminole Indians, vital data on the population of, *7*, 335
Sendai, Japan, paternity in, *2*, 508
Senile decay, *17*, 236
psychoses, in Negroes, *7*, 487
Senility, as "cause" of death, *4*, 97
biostatistics of, *11*, 14
Sensation-form theory of synesthesia, *5*, 168
Sense organs, diseases of, in Negroes, *3*, 228
Sensibility, primitive, reversion to, *1*, 418
Sensory-reflex theory of synesthesia, *5*, 179
Serological race classification, *2*, 128
Settlement, stage of, and birthplace of notables, *1*, 550
Severity of disease, and inheritance, *4*, 69
SEWARD, GEORGENE H., and SEWARD, JOHN P., JR., Changes in Systolic Blood Pressure, Heart Rate and Temperature Before, During and After Pregnancy in a Healthy Woman, *8*, 232-242
Seward, John P., Jr., *see* SEWARD, GEORGENE H.
Sex adjustment in marriage, *12*, 560
and handedness, *11*, 450
chromosomes, attached, and color blindness, *5*, 130
differences, anatomical, *15*, 127
in facial growth, *20*, 109
* in physical abilities, *19*, 12
* in pubic hair distribution, *17*, 137
differentials in mortality, Puerto Rico, *17*, 286

(Sex)
differentiation, human, head, trunk and appendages, *15*, 144
* factor in infant mortality, *1*, 90
influence on incidence of gastro-intestinal disease, *2*, 63
-influenced factors, gene-frequency analysis of, *7*, 608
-linked traits, *15*, 43
of Indian migrants, *15*, 43
of patients at Mayo Clinic, *2*, 185
organs, mortality in old age assignable to, *4*, 90, 103
ratio(s), and ancestral longevity, *12*, 404
and birth order, *20*, 187
and foetal mortality, *3*, 572; *10*, 235; *20*, 156
at birth, *4*, 272; *10*, 36
methods of studying, *15*, 255
correlation with growth-rate of population, *3*, 197
during war, *20*, 188
effect of family-size on, *20*, 185
in families, *9*, 99
German, *20*, 188
of college students, *3*, 346
in infant mortality, *3*, 560
in logistic population, *3*, 480
in Mexico, in 1793, *14*, 504
in Negroes, *4*, 11
in upper social strata, *20*, 182
* of abortions, a note on, *20*, 156
of Australian aborigines, *12*, 489
of Melanesians, *3*, 366
of San Juan Pueblo Indians, *12*, 152
of stillbirths and abortions, *10*, 235
of twins, *3*, 109
social factors affecting, *4*, 275
variations in, *14*, 85; *15*, 267
Sexual activity, of gorillas, *9*, 180
* behavior in the chimpanzee, *11*, 78
coldness, supposed, of early man, *5*, 57
continence, lethal effects of, *3*, 577
customs of New Ireland, *3*, 354
dimorphism, racial differences in, *15*, 132
history, in constitution study, *1*, 31
morality and the churches, *4*, 424
receptivity in chimpanzee, *11*, 110
selection, *10*, 72
as social selection, *1*, 253
SHANKLIN, WILLIAM M., and CUMMINS, HAROLD, Dermatoglyphics in Rwala Bedouins, *9*, 357-365
SHARP, R. LAURISTON, An Australian Aboriginal Population, *12*, 481-507
Sheerar, S. E., *see* YERUSHALMY, J.

*Sheldon's method for estimating dysplasia, *13*, 405
* and analysis of variance, *14*, 110
Shelesnyak, M. C., *see* ENGLE, E. T.
Shewart control chart technique, in industrial medicine, *19*, 31
Shoulder breadth, of schizophrenics, *11*, 364
width, growth of, *7*, 152
Shrinkage in height, diurnal, of children, *15*, 66
SHUTTLEWORTH, FRANK K., Good and Poor Years for the Growth of Girls in Stature and Body Weight, *12*, 280-286
Siamangs, growth and evolution of, *5*, 212, 385
relative growth in, *11*, 380
Sibs of longevous persons, *6*, 188
Siblings, resemblances in auditory acuity, *11*, 259
*Sick man, the, *1*, 445
*Sickle cell anemia and sicklemia, habitus of patients with, *16*, 99
*Sidelights on the population problem in Japan, *4*, 408
Sigma ratio, in anthropometric comparisons, *8*, 594
*Significance of the difference of two series of comparable means, *6*, 402
* of the variability of the upper lateral incisor teeth in man, *12*, 323
Simian primates, genitalia of, *8*, 309
SIMMONS, KATHERINE, Cranial Capacities by Both Plastic and Water Techniques with Cranial Linear Measurements of the Reserve Collection: White and Negro, *14*, 473-498
Simple monster, quantitative study of a, *2*, 1
*Simultaneous variations in the bodily and gastric activity of newborn infants under long-continued light stimulation (Relationship between bodily and gastric activity of newborn infants, II), *8*, 381
Sinanthropus, skull of, *2*, 439
Sitting-height index, and weight and lung volume prediction, *7*, 240
of different disease groups, *6*, 686; *8*, 65
of Japanese women, *14*, 292
of schizophrenic women, *14*, 29
Situs inversus viscerum, *12*, 324
Sivapithecus, dental characters of, *12*, 23
Size of family and permanence of residence, *19*, 112
Skeletal age and eyeball weight, in infants, *12*, 11
* and dental development and variability of the gorilla, *15*, 236, 307; *16*, 23
and muscular systems, mortality in old age assignable to, *4*, 91
* proportions of adult langurs and macaques, *14*, 444
Skeleton of the gorilla, *15*, 236, 307
* of the trunk and limbs of higher primates, *2*, 303
Skew variation, measurement of, *10*, 136
Skin color and climate, *2*, 244
disease in Negroes, *3*, 227
mortality in old age assignable to, *4*, 94
Skull(s) dimensions, and brain weight, *14*, 53
anatomic lag in, *6*, 526

(Skull(s) dimensions)
growth in, *14*, 143
of Indians, *14*, 181
of Negroes and whites, *14*, 492
* from Seleucia-on-the-Tigris dating from 100 B. C. to 200 A. D., *12*, 313
measurements of gibbons, *5*, 218
of the gorilla, *15*, 238, 307
primate, two types of, *1*, 216
Small sample problems, unsolved, *20*, 205
SMALLWOOD, W. M., Eugenics *circa* 590 B. C., *1*, 142-143
SNEDEKER, DORIS MANN, A Study of the Palmar Dermatoglyphics of Mongoloid Imbeciles, *20*, 146-155
SNYDER, LAURENCE H., "Laws" of Serological Race Classification, *2*, 128-133
and YINGLING, HAL C., Studies in Human Inheritance, XII. The Application of the Gene-Frequency Method of Analysis to Sex-Influenced Factors, with Especial Reference to Baldness, *7*, 608-615
Snyder, Laurence H., *see also* RIFE, DAVID C.
*Social class mortality, *17*, 1
classes, fertility of, *5*, 256; *6*, 595
condition, effect on race fusion, *15*, 155
disintegration in England and America, *4*, 247
factors affecting sex ratio, *4*, 275
and vital indices, *5*, 94
* promotion in relation to differential fecundity, *13*, 103
relationships of twins, *6*, 332
* selection and the formation of human types, *1*, 250
status, and personality disorders, *11*, 507
dimensions of newborn by, *6*, 616
trends of California and Nevada aborigines, *15*, 165
well-being and the churches, *4*, 421
Socio-economic status, and dental caries, *5*, 279
and growth, *19*, 68
and natality, *12*, 188
and permanence of residence, *19*, 105
birth order, and age of mother, *13*, 64
Sodeman, William A., *see* BURCH, GEORGE E.
Soft tissue volume, of finger tips, *10*, 295
Soil and birthplace of notables, *1*, 549
Sole prints of triplets, *5*, 11
*Somatic constitution of mothers with and without infant mortality among their progeny, observations on the, *12*, 232
* homogamy in man, *5*, 690
*Somatological differences associated with diseases of the heart in white males (Studies on constitution, II), *6*, 650
* norms, correlations, and changes with age (Biometric studies on U. S. Army officers), *4*, 509
* in disease (Biometric studies on U. S. Army officers), *5*, 61
* in tuberculosis and heart disease, *1*, 166

types, and endocrine weights, *8*, 92
in cardiac disease, *8*, 80
Somatology, constitutional, *1*, 283
* of the schizophrenic patient, *14*, 21, 192
*Some aspects of paternity in Sendai, Japan, *2*, 508
* data on fertility and economic status, *4*, 525
* effects of thyroid disorder in women, *3*, 529
* factors related to differential sex-ratios at birth, *4*, 272
* phases of human biology in Korea (Chosen), *3*, 322
* physiological phenomena fitted to growth equations. I. Moro reflex, *9*, 549; II. Brain potentials, *10*, 145
* quantitative evidence of the effect of thyroid disorder upon the birth rate, *5*, 516
* uses of the standard deviation in anthropometry, *8*, 592
SONTAG, L. W., and POTGIETER, MARTHA, The Variability of Nitrogen Excretion in Twenty-four Hour Periods as Compared with That in Longer Periods, *10*, 400-408
Souders, Helen J., *see* KELLY, HARRIET J.
Sources of cancer information, *16*, 119
SOUTHWICK, W. E., Autosomal Linkage between the Factors for the Production of Identical and Fraternal Twinning, *11*, 179-196
Span of different disease groups, *6*, 688
of Japanese women, *14*, 299
Specific fertility rates, Iowa population, *7*, 517
* gravity of the human body, *5*, 646
* vital indices for Japan, *1*, 198
SPENGLER, J. J., Messance: Founder of French Demography, *12*, 77-94
Spiegelman, Mortimer, *see* DUBLIN, LOUIS I.
Spotnitz, H., *see* ELSBERG, C. A.
*Spousal estimation of emotionality, *10*, 417
Spragg, S. D. Shirley, *see* CUMMINS, H.
*Spread of generations, *1*, 305
*Standard deviation in anthropometry, uses of, *8*, 592
* error of the slope of a straight line, *12*, 264
therapy rates among white and Negro mental patients, *11*, 513
Standardization of death rates, *18*, 127, 238
Stanton, Frederick L., *see* GOLDSTEIN, MARCUS S.
Stanton, R. G., *see* BEALL, GEOFFREY
*Stationary population, a (A study of Iowa population), *7*, 514
*Statistical approach to the problem of tone localization in the human cochlea, *6*, 714
* approaches to some problems of industrial preventive medicine, *19*, 27
* contributions from the Mental Hygiene Study of the Eastern Health District of Baltimore. III, and IV. Personality Disorder in the Eastern Health District in 1933, *11*, 112, 485
* diagnosis of zygosity in multiple human births, *15*, 221
* phenomenon involving hair-color and physical types, *13*, 541

Stature, *see also* Body length, Height
anatomic lag in, *6*, 531
and blood characters, *2*, 277
and growth of extremities, *9*, 400
average and annual increment, of girls, 11-13 years, *17*, 233
gain in, of boys 8-13 years, *10*, 289
good and poor years for growth in, *12*, 280
growth curves and formulas, Chinese, *15*, 9
individual growth in, *13*, 319
of army officers, *4*, 510
of children, white, *12*, 282
Negro, *10*, 129
of college women, *5*, 122, 600; *11*, 220
of different disease groups, *6*, 670
of Japanese women, *14*, 288
of Lapps, *14*, 382
of men students, *5*, 445
of schizophrenic women, *14*, 29
of Swedes, *14*, 382
* of Toronto children half a century ago and today, *16*, 127
of West African Negroes, *9*, 483
prediction of, for schoolboys of North European stock, *8*, 279
seasonal cycle in growth in, *2*, 523
Status lymphaticus, *4*, 166
Steatopygia, as an adaptive character, *2*, 243
Stefansson, Vilhjalmur, *see* SELTZER, CARL C.
STEGGERDA, MORRIS, Form Discrimination Test as Given to Navajo, Negro and White School Children, *13*, 239-246
Steggerda, Morris, *see also* WOLFF, GEORG
Sterility of adolescence, *8*, 607
Sternum of gibbons, *5*, 243
of gorillas, *16*, 26
of langurs and macaques, *14*, 448
of primates, *2*, 329
STEVENSON, P. H., SUNG, S. M., PAI, T., and LYMAN, R. S., Chinese Constitutional Differentiation and Kretschmerian Typology, *9*, 451-482
Stillbirth(s), age at first, *9*, 84
causes of, *10*, 53, 235
in Negroes, *3*, 233; *4*, 12
in twins, factors causing, *12*, 259
parity of, by age of mother, *9*, 213
rates, by age of mother and order of birth, *17*, 73, 95
by age of parents, *11*, 344
effect of interval between births on, *17*, 71
sex factor in, *1*, 92
ratio of, *4*, 273; *10*, 235
STONE, CALVIN P. and BARKER, ROGER G., On the Relationship between Menarcheal Age and Certain Measurements of Physique in Girls of the Ages 9 to 16 Years, *9*, 1-28

Stone, Calvin P., *see also* BARKER, ROGER G.
STRANDSKOV, HERLUF H., On the Variance of Human Live Birth Sex Ratios, *14*, 85-94
and DIEDERICH, GERTRUDE WYLIE, The Rh Blood Factor among Twins, *17*, 195-206
STRAUS, WILLIAM L., JR., The Phylogeny of the Human Forearm Extensors, *13*, 23-50, 203-238
Strength, sex differences in growth in, *19*, 12
*Structure of a growing population, the, *3*, 459
"Studentization," *20*, 205
*Studies in human inheritance. VI. A genetic refutation of the principles of "behavioristic" psychology, *3*, 547; XII. The application of the gene-frequency method of analysis to sex-influenced factors with especial reference to baldness, *7*, 608; XVI. Distribution of sex-ratios within families in an Ohio city, *9*, 99
* in the physical development. V. The ossification time of the os pisiforme, *17*, 143; VI. The chronology of physiologic events, *17*, 147; VII. Environmental trends among the American Negro, *17*, 207; VIII. Resumé and goals for further research, *17*, 229
* on constitution. I. Methods, *1*, 10; II. Somatological differences associated with diseases of the heart in white males, *6*, 650; III. Somatological differences associated with diseases of the heart in white females, *8*, 38; IV. Endocrine organ weights and somatological habitus types, *8*, 92
* on human longevity. IV. The inheritance of longevity, *3*, 245; V. Constitutional factors in mortality at advanced ages, *4*, 80; VI. The distribution and correlation of variation in the total immediate ancestral longevity of nonagenarians and centenarians, in relation to the inheritance factor in duration of life, *6*, 98
* on pupils of the Pennsylvania School for the Deaf. 2. Resemblances in the auditory acuity of siblings, *11*, 259
* on the biological factors in public health. I. The trend of the age of marriage in Washington County, Maryland, from 1897 to 1938, *12*, 59
* on twins. I. The relation of order of birth and age of parents to the frequency of like-sexed and unlike-sexed twin deliveries, *12*, 95; II. On the early mortality of like-sexed and unlike-sexed twins, *12*, 247
*Study of factors associated with the growth and nutrition of Porto Rican children, *4*, 469
* of fetal movements in relation to mother's activity, *18*, 221
* of four skulls from Seleucia-on-the-Tigris dating from 100 B. C. to 200 A. D., *12*, 313
* of like-sexed twins. I. Vital statistics and familial data, *3*, 107; II. Their health and disease records, *3*, 270
* of physiological factors: the autonomic nervous system and the skeletal musculature, *14*, 69
* of twins with special reference to heredity as a factor determining differences in environment, *6*, 324
* on the frequency of left-handedness and left-footedness among Japanese school children, *6*, 33

Stutterers, ancestry of, *11*, 393
twins, *9*, 329
Suicides, and weight, *2*, 182
Sulfonamides, kinetics of absorption and excretion of, *18*, 171
Sung, T. M., *see* STEVENSON, P. H.
*Supernumerary nipples, congenital hemihypertrophy, and congenital hemiatrophy, *11*, 447
Supine and erect body length, *4*, 262
Suprarenal glands, weight of, *6*, 508
Supreme Court, agreement of Justices of, *19*, 212
Surface-area of body, and blood characters, *2*, 277
of finger-tip, *11*, 538
of human body, *10*, 366
Survival of offspring, and age of father, *11*, 342
rate(s), differential, *1*, 508
generation, *19*, 133
Survivorship and chronic sickness, *18*, 33
curves, Puerto Rico and the United States, *17*, 285
* in aboriginal populations, *19*, 83
in closed population groups, *18*, 158
Suture(s) closure, in gorilla, *15*, 309
supernumerary, in gorilla, *15*, 316
Swinging of arms, function of, in walking, *11*, 534
Symmetry, dactyloglyphic, *10*, 91, 491
Symphalangus, *5*, 213, 386, 416
Syndactyly, and apical dystrophy, *4*, 191
and metopism, *17*, 133
*Synesthesia, theories of, in the light of a case of color-hearing, *5*, 155
Synkinesis, in brain disease, *1*, 412
Syphilis, in Negroes, *3*, 93; *17*, 217
cerebral, *7*, 495
infants, *4*, 24
families with, in Eastern Health District, *15*, 112
in the insane, *4*, 212
Syracuse, New York, fertility study, *6*, 595
*Systolic and diastolic blood pressure in healthy men, *5*, 541
SYZ, HANS, The Concept of the Organism-as-a-Whole and Its Application to Clinical Situations, *8*, 489-507
VON SZELISKI, VICTOR S., Population Growth Due to Immigration and Natural Increase, *8*, 25-37

Taback, Mathew, *see* ZUBIN, JOSEPH
Tail, loss of, in primate evolution, *2*, 235
Tanala, social selection among, *1*, 258
TANDY, ELIZABETH CARPENTER, Secular Changes in Mortality Rates Connected with Certain Organ Systems, *3*, 494-528

Tarsier-monkey phylum, *1*, 224
Tarsius, development of hair tracts in, *12*, 205
Taste reactions, of Baghdad population, *13*, 400
of Gentile and Jewish students, *16*, 175
TAYLOR, GRIFFITH, Races of the World: A Discussion of Recent Classifications, *13*, 390-397
Racial Migration-zones and Their Significance, *2*, 34-62
The Zones and Strata Theory—A Biological Classification of Races, *8*, 348-367
Technique, measuring cranial capacity, *14*, 475
measurement of pigmentation, *1*, 322
Teeth, dehiscence of, *2*, 483
development of, and growth, *5*, 438
eruption, in ancient and modern Greeks, *16*, 285
inherited reductions in, *6*, 627
lateral incisors, significance of variability of, *12*, 323
movement of, in children, *8*, 161
of gibbons, *5*, 235
of identical twins, *5*, 629
polyisomerism in, *6*, 635
transition from deciduous to permanent, *8*, 179
wear of, in gorilla, *15*, 314
Temperament of gorillas, *9*, 193
Temperature changes in the menstrual cycle, *4*, 453
during pregnancy, *8*, 233
oral and rectal, *7*, 24
Temporal muscle area and cranial characters, *2*, 239
Tenerife crania, *6*, 309
Tennessee mountain children, physical status of, *5*, 706
*Teratology, quantitative study in, *2*, 1
Territorial expansion growth curves, *18*, 204
Testes, weight of, *6*, 511; *7*, 574
Tetanus in Negroes, *3*, 93
in Negro infants, *4*, 26
Theognis, and eugenics, *1*, 142
*Theories of synesthesia in the light of a case of color-hearing, *5*, 155
Thiamin, sources of, and appetite levels, *15*, 210
Thigh circumference, growth of, *9*, 376
*Third aspect of growth, a, *6*, 405
THOMPSON, HELEN, A Third Aspect of Growth, *6*, 405-407
THOMPSON, WARREN S., Natural Selection in the Processes of Population Growth, *1*, 503-513
Thoracic circumference, of Japanese women, *14*, 311
* index, the, *6*, 1
Throat diseases in Negroes, *3*, 215
Thumbs, doubling of, and apical dystrophy, *4*, 191
Thymus, weight of, *6*, 503; *7*, 585
Thyroid disorder and birth-rate, *5*, 516

(Thyroid disorder)
in women, effects of, *3*, 529
weight of, *6*, 499; *7*, 383
TIAL—total immediate ancestral longevity, *6*, 98
Tibia of primates, *2*, 367
TIETZE, CHRISTOPHER, A Note on the Sex Ratio of Abortions, *20*, 156-160
Tietze, Christopher, *see also* COHEN, B. M.
Time discrimination, and race, *1*, 75
Tobacco and blood pressure, *5*, 573
Todd, Arthur W., *see* TODD, T. WINGATE
TODD, T. WINGATE, Entrenched Negro Physical Features, *1*, 57-69
BEECHER, HARRY, WILLIAMS, GUY H., and TODD, ARTHUR W., The Weight and Growth of the Human Eyeball, *12*, 1-20
Toe prints, and finger prints, *8*, 531
projection, in Negroes and whites, *16*, 209
tip volume, *13*, 526
Tone localization in cochlea, *6*, 714
Topography and birthplace of notables, *1*, 548
of the human body, *17*, 314
Torpin, Richard, *see* DOW, PHILIP
Total immediate ancestral longevity (TIAL), *6*, 98
* volume of the human finger tip and toe tip, a study of, *13*, 526
Traits, indicial and metric, in gorilla, *15*, 320
Transient group in the Eastern Health District, *15*, 108
Transitory asymmetry of the vertebral column, *16*, 155
*Transverse growth of the extremities: an analysis of girth measurements for arm, forearm, thigh and leg, taken on Iowa City white children, *9*, 366
Tree shrew-lemur phylum, *1*, 223
Treloar, Alan E., *see* GUNSTAD, BORGHILD
*Trend(s) in the fertility of social classes from 1900 to 1910, *5*, 256
* in the proportional contribution of the socio-economic groups to natality, *12*, 188
* of the age of marriage in Washington County, Maryland from 1897 to 1938 (Studies on the biological factors in public health, I), *12*, 59
Triplets, identical, *5*, 1
Triradius, axial and digital, in chimpanzee, *10*, 477
Trochanter height, of Japanese women, *14*, 296
TROTTER, MILDRED and LANIER, PATRICIA F., Hiatus Canalis Sacralis in American Whites and Negroes, *17*, 368-381
Trunk length, of different disease groups, *6*, 687; *8*, 65
measurements of, in Negroes, *15*, 128
width, development of, *7*, 151
t-test, *20*, 205
*Tuberculosis and civilization: Part I. Basic facts and figures in the epidemiology of tuberculosis, *10*, 106; II. Interpretation of the etiological factors in the epidemiology of tuberculosis, *10*, 251
and height-weight, *1*, 166
and natural selection, *10*, 261

hereditary factors in, *10*, 255
in Negroes, *3*, 95; *17*, 219
infants, *4*, 25
in the insane, *4*, 209
mortality, by country of origin, *17*, 353
pulmonary, and weight, *2*, 176
* prevalence among selectees, *16*, 181
somatology of, *1*, 166; *5*, 62
social factors of, *10*, 261
* syphilis and cancer (Biometrical studies in psychiatry, VII), *4*, 208
Tuberculous families, Eastern Health District of Baltimore, annual rates of migration of, *15*, 109
TUKEY, JOHN W., Some Elementary Problems of Importance to Small Sample Practice, *20*, 205-214
Tularemia, seasonal occurrence in North Central States, *13*, 334
Tully, Mildred R., *see* LOMBARD, HERBERT L.
Tumors of digestive tract, and sex, *2*, 90
Tungus, blood groups of, *8*, 273
Turko-Tatars, blood group B in, *14*, 433
*Twelve-day old baby, the, *8*, 433
Twinning, dizygotic and monozygotic, genetic factors for, *11*, 182
in stuttering families, *9*, 331
Twins, agglutinogens M and N in, *7*, 237
births among San Juan pueblo Indians, *12*, 162
births in Negroes, *4*, 9
conjoined, *situs inversus viscerum* in, *12*, 69
deliveries, by age of parents, *12*, 99
by sex, *12*, 252
dermatoglyphics of, *10*, 12; *15*, 48
diagnosis of zygosity, *3*, 114; *5*, 29; *10*, 12; *15*, 221
dizygotic, stillborn, *12*, 251
handedness, *9*, 340; *15*, 48
heredity of, as factor in differences in environment, *6*, 324
in *Who's Who*, *5*, 371
like-sexed, familial histories of, *3*, 107
health record of, *3*, 270
mirror-imaging in, *6*, 24; *12*, 21
monozygotic, *11*, 180; *15*, 55
dentition of, *5*, 629
dermatoglyphics of, *10*, 21
discordance in, *19*, 217
malocclusion in a pair of, *20*, 36
stillborn, *12*, 251
* mortality of like- and unlike-sexed, *12*, 247
premature birth of, *12*, 257
Rh blood factor among, *17*, 195
social relations of, *6*, 332

(Twins)
stillbirth rate of, *12*, 249
*Two unlike expressions of symmetry of finger tip patterns, *10*, 77
Typhoid fever, and weight, *2*, 176
Typology, Chinese, *9*, 451

Ulcers of esophagus, stomach and duodenum, and sex, *2*, 77
Underweight, and causes of death, *2*, 159
Unilateral defects of body, *11*, 465
United States and New World populations, *12*, 400
longevity in, *6*, 43
Unreality, feelings of, *1*, 514
Upper classes, are they superior? *1*, 510
extremities, of children, *11*, 277
face height, morons, *13*, 465
Urban and rural death rates, *3*, 504
mortality sex ratios, *1*, 102
sex ratios, *10*, 43
children, dental caries in, Puerto Rico, *5*, 275
fertility, 1900-1910, *5*, 256
Urine, nitrogenous constituents of, and protein intake, *7*, 419
*Use of iconometrography in graphic exposition. I. Topography and composition of the human body, *17*, 314
* of indices as an auxiliary method in the establishment of physical types, *3*, 420
* of life table death rates for comparative mortality, *18*, 127, 328
* of the logistic curve to represent the prevalence of defective vision among persons of specific ages above 30 years, *7*, 257
Uterus, weight of, *6*, 517
Uzbeks, blood group B in, *14*, 437

VALAORAS, VASILIOS G., A Comparative Study of the Mortality of the Population of Greece, *8*, 553-564
Valvular heart disease, somatology of, *5*, 69
VAN DUSEN, C. R., An Anthropometric Study of the Upper Extremities of Children, *11*, 277-284
Variability, cranial and dental, in gorilla, *15*, 313
determined by iconometrographic method, *17*, 325
in Eurafrican juveniles, *11*, 332
in girth of extremities, *9*, 378
in human foot length, *16*, 230
in hybrid and purer stocks, *1*, 66
in pigmentation, *1*, 350
in weight of infants, changes in, *11*, 54
of cranial index, *13*, 95
of long bones in primates, *9*, 303
*Variability of nitrogen excretion in twenty-four hour periods as compared with that in longer periods, *10*, 400
of the gorilla, *15*, 313; *16*, 23

* of the percentage index of build as applied to the prediction of normal weight, *11*, 473
* of the upper lateral incisors in man, significance of, *12*, 323
of time of occurrence of physiologic events, *17*, 147
Variables, physiological, intercorrelations between, *14*, 75
*Variance of a grouped frequency distribution when the mean of each class is taken as centering point, *6*, 561
* of human live birth sex ratios, on the, *14*, 85
of sex ratios, *15*, 267
sample, *20*, 205
Variation(s) bimanual, in dermatoglyphics, *14*, 523
* in parity of women bearing children in the U. S. Birth Registration Area in 1930, *9*, 65, 212
in the sex ratio at birth in the United States, *10*, 36
* intracranial, in brain weight, *14*, 48, 235
of gorilla skeleton, *16*, 40
of weight with age, predictions of, *11*, 405
range of, measurements of non-schizophrenic and schizophrenic women, *14*, 32
Vascular disease and finger tip volume, *11*, 540
Velocities of growth, *9*, 389; *15*, 19
of skull, in time, *14*, 148
of joint movements, measurement of, *9*, 203
*Venner, Tobias, and his *Via Recta*, *4*, 558
*Verhulst, Pierre-Francois, the Discoverer of the Logistic Curve, *5*, 673
Verhulst-Pearl logistic curve, *10*, 2
Vertebrae, number of, in primates, *2*, 309
of gibbons, *5*, 239
Vertebral column asymmetry, *16*, 153
measurements, of gorilla, *16*, 24
Via Recta ad Vitam Longam, *4*, 558
Vibration theory of synesthesia, *5*, 163
Violent deaths of Negroes, *3*, 234
VISHER, STEPHEN S., Ecology of American Notables, *1*, 544-554
VISHNEVSKY, B. N., Blood Groups of Tungus, *8*, 273-278
Vision, disturbance of, in petit-mal attacks, *2*, 547
prevalence of defective, represented by logistic curve, *7*, 257
*Visual acuity of selectees and army inductees, *16*, 1
*Vital data on the population of Seminole Indians of Florida and Oklahoma, *7*, 335
factors, correlation with growth-rate of population, *3*, 180
* function studies. XIII. The influence of abnormal sitting height indices on weight and lung volume predictions, *7*, 240
* history of San Juan pueblo, *12*, 141
index, correlation with growth-rate of population, *3*, 187
of Korea, *3*, 327
of various populations, *12*, 376
* indices and their relation to psychological and social factors, *5*, 94
for Japan, *1*, 198

(Vital)
* statistics of New Ireland (Bismarck Archipelago) as revealed in genealogies, *3*, 351
VOLTERRA, VITO, Calculus of Variations and the Logistic Curve, *11*, 173-178
Population Growth, Equilibria, and Extinction under Specified Breeding Conditions. A Development and Extension of the Theory of the Logistic Curve, *10*, 1-11
Volume contour maps, *10*, 357
correlations of finger tip and surface area, *11*, 536
lung, and sitting height index, *7*, 240
of finger tip, *10*, 295
and toe tip, *13*, 526

Wage-earners in family, and permanence of residence, *19*, 114
Walking movements, and arm movements, *11*, 531
Wallin, Paul, *see* BURGESS, ERNEST W.
WALLIS, RUTH SAWTELL, Cranial Relationships and Correlations, *6*, 308-323
WALLIS, WILSON D., Anatomic Lag, *6*, 524-542
Anatomic Lag in Head and Body Dimensions, *13*, 473-504
War and population, pre-colonial Mexico, *18*, 81
years and childhood mortality, *10*, 537
WASHBURN, SHERWOOD L., Skeletal Proportions of Adult Langurs and Macaques, *14*, 444-472
Watkins, J. H., *see* ABERLE, S. D.
Wealth distribution and birthplace of notables, *1*, 547
per capita and the churches, *4*, 421
Weather and health, *10*, 388
Weight and age relationship, *11*, 274
and blood pressure, *5*, 567
* and growth of the human eyeball, *12*, 1
and increase in weight, correlations, *19*, 260
and physical fitness, *15*, 295
and sitting height index, *7*, 240
* and stature of Negro city children of adolescent age, a study of, *10*, 124
gain in, association with ancestry, and economic status, *12*, 532
index, and season of birth, *13*, 537
individual growth curves, *13*, 318
influence on causes of death, *2*, 159
loss, variability in infants, *11*, 54
of army officers, *4*, 512
of brain, human, *14*, 48, 235
of college men, *5*, 445
of Eurafrican juveniles, *11*, 325
of "fat" people, *14*, 166
of girls, *9*, 26; *10*, 127; *13*, 318
of gorillas, *9*, 178
of infants, *10*, 409; *11*, 26, 248
Negro, at birth, *4*, 12

of mental patients, *7*, 366
of Negroes, Tuskegee, *15*, 132
of placenta, *11*, 248
of schizophrenics, *11*, 365
of school children of two socio-economic groups, *19*, 68
* of the endocrine glands (Biometrical studies in psychiatry, VIII), *6*, 489
* of the parathyroids, *9*, 245
of women, American college, *5*, 122, 600
Japanese, *14*, 316
postnatal gain and loss of, *11*, 49
prediction of, from height, *7*, 243
sex differences in growth, *19*, 12
WEINBACH, A. P., Contour Maps, Center of Gravity, Moment of Inertia and Surface Area of the Human Body, *10*, 356-371
Some Physiological Phenomena Fitted to Growth Equations. I. Moro Reflex, *9*, 549-555; II. Brain Potentials, *10*, 145-150
WENGER, M. A., A Study of Physiological Factors: The Autonomic Nervous System and the Skeletal Musculature, *14*, 69-84
West African Negroes, finger tip patterns of, *10*, 86
physical types of, *9*, 483
WESTPHAL, KURT, The Use of Indices as an Auxiliary Method in the Establishment of Physical Types, *3*, 420-428
WHEELER, LESTER R., A Comparative Study of the Physical Status of East Tennessee Mountain Children, *5*, 706-721
WHELPTON, P. K., and JACKSON, NELLE E., Prolificacy Distribution of White Wives According to Fertility Tables for the Registration Area, *12*, 35-58
Whitechapel crania, *6*, 316
Whooping cough in Negroes, *3*, 89; *4*, 24
Whorls, frequencies of, and handedness, *14*, 518
Who's Who, twins and brothers in, *5*, 371
WIENER, ALEXANDER S., Heredity of the Agglutinogens M and N of Landsteiner and Levine. IV. Additional Theoretico-Statistical Considerations, *7*, 222-239
and ROTHBERG, SIDNEY, Heredity of the Subgroups of Group A and Group AB, *5*, 577-586
Williams, Guy H., *see* TODD, T. WINGATE
WILLOUGHBY, DAVID P., An Extraordinary Case of Obesity and a Review of Some Lesser Cases, *14*, 166-177
WILLOUGHBY, RAYMOND ROYCE, Comments on the Unhappiness of Mr. Fraser Roberts, *14*, 113-114
Note on the Death Rate among Persons Receiving Old-Age Assistance in Rhode Island, *14*, 386-388
Somatic Homogamy in Man, *5*, 690-705
Spousal Estimation of Emotionality, *10*, 417-425
The Amalgamation of Ethnic Groups, *7*, 437-442
The Families of Freshmen, *3*, 342-350
and COOGAN, MARGUERITE, The Correlation between Intelligence and Fertility, *12*, 114-119

WILMER, HARRY A., and SCAMMON, RICHARD E., The Use of Iconometrography in Graphic Exposition. I. Topography and Composition of the Human Body, *17*, 314-339
Wilson, J. W., *see* ROBINSON, S.
WILSON, PAUL T., A Study of Like-sexed Twins. II. Their Health and Disease Records, *3*, 270-281
A Study of Twins with Special Reference to Heredity as a Factor Determining Differences in Environment, *6*, 324-354
and JONES, HAROLD E., A Study of Like-sexed Twins. I. Vital Statistics and Familial Data, *3*, 107-132
WINSLOW, C.-E. A., and HOLLAND, DOROTHY F., The Influence of Certain Public Health Procedures upon Infant Mortality, *9*, 133-174
WINSOR, CHARLES P., "Das Gesetz der kleinen Zahlen," *19*, 154-161
Factorial Analysis of a Multiple Dichotomy, *20*, 195-204
"Probability and Listerism," *20*, 161-169
Winsor, Charles P., *see also* LASKER, GABRIEL W.
WINSOR, TRAVIS, and BURCH, GEORGE E., The Habitus of Patients with Sickle Cell Anemia, *16*, 99-114
WINSTON, SANFORD, Some Factors Related to Differential Sex-ratios at Birth, *4*, 272-279
WINTROBE, M. M., Correlation of Characters of the Blood with Body-weight, Stature and Surface-area, *2*, 277-291
WISLOCKI, GEORGE B., The External Genitalia of the Simian Primates, *8*, 309-347
WISSLER, CLARK, Distribution of Deaths among American Indians, *8*, 223-231
WOLFE, A. B., The Fecundity and Fertility of Early Man, *5*, 35-60
WOLFF, GEORG, Tuberculosis and Civilization. Part I. Basic Facts and Figures in the Epidemiology of Tuberculosis, *10*, 106-123; Part II. Interpretation of the Etiological Factors in the Epidemiology of Tuberculosis, *10*, 251-284
and STEGGERDA, MORRIS, Female-Male Index of Body Build in Negroes and Whites: an Interpretation of Anatomical Sex Differences, *15*, 127-152
WOOD-JONES, FREDERIC, Phylogeny of the Primates, *1*, 214-228
Wood-Jones, Frederic, critique of his paper, *2*, 99
WOOFTER, T. J., Completed Generation Reproduction Rates, *19*, 133-153
Probabilities of Death in Closed Population Groups, Illustrated by Probabilities of Death of White Fathers after Birth of Children, *18*, 158-170
Working women, anthropometric standards for, *6*, 472
hand size of, *8*, 267
*World population growth, *8*, 397
Wormian bones, *18*, 61
Wrist, ossification of, *6*, 454

X-rays of teeth of identical twins, *5*, 634
of triplets, *5*, 17

Yeast cells, distribution of, *10*, 138
YERKES, R. M., Sexual Behavior in the Chimpanzee, *11*, 78-111
YERUSHALMY, J., Age of Father and Survival of Offspring, *11*, 342-356

On the Interval between Successive Births and Its Effect on Survival of Infant. I. An Indirect Method of Study, *17*, 65-106
and SHEERAR, S. E., Studies on Twins. I. The Relation of Order of Birth and Age of Parents to the Frequency of Like-sexed and Unlike-sexed Twin Deliveries, *12*, 95-113; II. On the Early Mortality of Like-sexed and Unlike-sexed Twins, *12*, 247-263
Yingling, Hal C., *see* SNYDER, LAURENCE H.
YUAN, I-CHIN, The Influence of Heredity upon the Duration of Life in Man Based on a Chinese Genealogy from 1365 to 1914, *4*, 41-68
Life Tables for a Southern Chinese Family, *3*, 157-179

ZAPP, JOHN A., JR., Statistical Approaches to Some Problems of Industrial Preventive Medicine, *19*, 27-52
ZIEGLER, LLOYD H., Compulsions, Obsessions and Feelings of Unreality, *1*, 514-527
"Zones and strata" concept, *2*, 35
*Zones and strata theory; a biological classification of races, *8*, 348
ZUBIN, JOSEPH, and TABACK, MATHEW, A Note on Sheldon's Method for Estimating Dysplasia, *13*, 405-410
ZUCKER, LOIS M., Evaluation of Slopes and Intercepts of Straight Lines, *19*, 231-259
Zygosity diagnosis, in multiple births, *3*, 114; *5*, 29; *10*, 12; *15*, 221
of triplets, *15*, 228

HUMAN BIOLOGY

a record of research

DECEMBER, 1948

Vol. 20 No. 4

PUBLISHED QUARTERLY BY
THE JOHNS HOPKINS PRESS, BALTIMORE, MARYLAND, U.S.A.

HUMAN BIOLOGY

a record of research

FOUNDED BY RAYMOND PEARL

Edited by

CHARLES P. WINSOR

in association with

A. W. FREEMAN A. D. LANGMUIR ADOLF MEYER
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FRANCES D. NICOLLS

HUMAN BIOLOGY is published quarterly by The Johns Hopkins Press, Baltimore, Maryland, U. S. A. The advance subscription price is $5.00 a year (foreign postage 35 cents; Canadian 13 cents). Single current issues are $2.00 each; back numbers, $3.00 each; back volumes, $6.00; plus postage. Subscriptions for less than a year are charged at the single issue price.

Subscribers should notify the publishers of change in address at least two weeks before publication of the issue with which change is to take effect. No claim for non-receipt of an issue will be entertained unless made immediately upon receipt of the next succeeding issue.

Manuscripts and communications regarding editorial matters should be addressed to Editor, Human Biology, 615 N. Wolfe St., Baltimore—5, Maryland, U. S. A.

THE JOHNS HOPKINS PRESS, *Publishers*
BALTIMORE—18, MARYLAND, U. S. A.

Entered as Second Class matter, February 18, 1929, at the Post Office at Baltimore, Md., under the Act of March 3, 1879.

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Human Biology

a record of research

VOLUME 20 DECEMBER, 1948 NUMBER 4

CONTENTS

THE RELATION BETWEEN INBREEDING AND INTELLIGENCE. *S. A. Asdell* 171

RECENT CHANGES IN THE SECONDARY SEX RATIO OF THE UPPER SOCIAL STRATA. *Marianne E. Bernstein* 182

FACTORIAL ANALYSIS OF A MULTIPLE DICHOTOMY. *Charles P. Winsor* 195

NOTES: SOME ELEMENTARY PROBLEMS OF IMPORTANCE TO SMALL SAMPLE PRACTICE. *John W. Tukey* 208

INDEX TO VOLUME XX 9

INDEX TO VOLUMES I TO XX 217

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BALTIMORE—18, MARYLAND, U. S. A.